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SUPPLEMENTAL SCIENCE AND PERFORMANCE ANALYSES (SSPA)

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Las Vegas, Nevada 89109

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PROCEEDINGS

8:00 a.m.

BULLEN: Good morning. Thank you for returning for this morning's session, and I'd like to ask you to grab your cup of coffee and take a seat.

Before I turn the session over to Dr. Sagüés to run this technical session, I have a couple of housekeeping announcements that I'd like to make.

First and foremost, you'll notice that the agenda has a public comment period scheduled for this afternoon, and I've had a couple of members of the public who are interested in making a comment but cannot stay until the end of our meeting. So we're going to try and adjust the schedule just a little bit, so what I'd like to do is immediately prior to lunch, and hopefully that will occur right at noon, we will have a 15 minute public comment period.

Again, if you would like to make a comment, sign up with one of the Lindas at the back table in the back of the room. And if you can wait until the end of the day, I think I might be able to allow you a little bit more time. But I'm going to keep the comment period at noon to 15 minutes. So if you need to comment and cannot stay until the end of the meeting, I'd like you to sign up for the noon comment period. If you can wait until later in the day, I would appreciate it, and we'll give you the allotted time then.

I'm still planning to close the meeting right at 5 o'clock, so we'll try and adjust the comment periods accordingly.

That's the only housekeeping measure I have. I know that Alberto Sagüés would like to make a brief statement about a workshop that the Board is sponsoring, and I will turn it over to Alberto, at which point we will begin this morning's session. Alberto?

SAGÜÉS: Thanks, Dan. Good morning. My name is Alberto Sagüés, and I'm a member of the Nuclear Waste Technical Review Board. I would like to welcome you to this morning's session of the joint meeting of the Board's Performance Assessment and Repository Panels devoted to DOE's Supplemental Science and Performance Analyses or the SSPA Report.

For most of the morning, we will hear presentations on the scientific work done in different technical areas. Greg Gdowski of Lawrence Livermore National Laboratories and Joon Lee of Sandia National Laboratories will make presentations on waste package corrosion process components.

They will be followed by Pat Brady of Sandia National Laboratories, who will talk about the waste form process components, Jim Houseworth of Lawrence Berkeley National Laboratories, who will talk about unsaturated zone process components, and Bruce Robinson of Los Alamos National Laboratory, who will talk about saturated zone process components.

The rest of this morning will be given by Bob Andrews of Bechtel SAIC, who will tell us how all the different technical information we have been hearing about will be incorporated into the Supplemental Total System Performance Assessment.

Before I give the meeting to the speakers, I wanted to make a brief announcement, which I may repeat later in the day, and that has to do with a workshop, an international workshop on long-term extrapolation of passive behavior that the Nuclear Waste Technical Review Board will be hosting on July 19th, Thursday, and July 20th, Friday morning, and that will take place in Arlington, Virginia.

And in that workshop, we will be discussing the important issue of taking, over a very long time frame, the kind of findings that are developed on a more limited time by means of laboratory experiments, and also looking at the engineering experience. There is a sheet with an announcement concerning this workshop right there on the table.

Having said that, I'm going to turn over the meeting to Greg Gdowski and Joon Lee. And the floor is yours.

GDOWSKI: I'm going to talk about five topics that are related to the waste package degradation, and each one of these topics I'm going to present what we attempted to do in TSPA-SR, and then I will talk about how these things have changed and how we're implementing them in SSPA. These are the five topic areas.

The first one is environmental concerns. Concerning the range of water chemistries that can contact the waste packages, in TSPA-SR, we considered that only carbonate base brines would be contacting the waste packages. That is, all the aqueous solutions would be composed of sodium salts, and this has implications which I will discuss in the next slide.

Now we also consider that near neutral pH brines can be in contact with the waste packages. In particular, what this means is that calcium and magnesium salts may be present in these solutions. The reason they're not present in the carbonate base brines is because of precipitation of insolubles carbonates of silicates.

Another effect that we considered in SSPA was the presence of lead, soluble lead in waters that may contact the waste packages.

What we have done is a literature review of lead in dilute water systems, showing that they're limited by the precipitation of salt based in carbonates. We also reviewed the USGS database on lead solubility in waters that encompass the Yucca Mountain region, and showed that the median concentration in these solutions is about 9 ppd.

We also have an ongoing experimental program to assess the amount of lead that can remain in solutions that are undergoing evaporative concentration. We also are updating the thermodynamic databases so that we can perform calculations to understand how lead would evolve in these solutions. We are also looking at arsenic and mercury under the same sort of conditions.

On a related topic, we're also considering now the aspects of redox couples in these solutions, and how they may affect the electrochemical potentials in these solutions. We're considering not only elemental species redox couples, such as ferric/ferrous, for lead plus two/lead metal system, but also the molecular species, the nitrates and the sulfate solutions.

We're also considering that there are other sources of soluble salts that can contact the waste packages. These include the entrained matter that may be in the ventilation system, and also rock dust. We have an ongoing program with the USGS that is evaluating both of these aspects.

This slide is a plot of the deliquescence points for pure salts. A deliquescence point is a relative humidity that defines where we can form aqueous solutions. Any relative humidity at the deliquescence point and above it will form an aqueous solution for that particular salt.

If we consider a salt like sodium chloride has a deliquescence of 75 percent, what that means is that any relative humidity above 75 percent, we form aqueous solutions with that particular salt. Below 75 percent relative humidity, the salt exists as a dry salt.

In TSPA-SR, what we considered was that we had only sodium salts present. We defined the deliquescence point in that system by the most deliquescence point in salt that we might have in those solutions, and that was sodium nitrate. You can see that that decreases, the deliquescence point decreases from about 70 percent at 50 degrees C., down to its boiling point, which is about 50 percent at 120 degrees C. So we considered a range of aqueous solution formation in this plot above that line.

For SSPA, we also now considered that we may have magnesium or calcium chloride type salts on the waste packages. What that does is increases the range that we might expect to have aqueous solutions on these waste packages. Considering mag. chloride, we see that it has a deliquescence point of about 31 percent at 50 degrees C. Extrapolating out to the boiling point for calcium chloride indicates that about 165 degrees C. has a deliquescence point of around 15 percent. So our range at which we can form aqueous solutions on the waste packages has increased significantly.

We now consider phase stability in the Supplemental Science and Performance Analyses. Our new data increases our confidence in TSPA. To support our rather short-term extrapolation of experimental data to long-time, we have theoretical modelling that shows that phase instabilities under repository conditions for the base metal are not expected. I have a backup slide that shows some of this data that I won't go into.

We also have shown that long-range ordering is not expected to be a problem for 300 degrees C. Previously, our data had indicated that it wasn't a problem below 260 degrees C. So we've increased our margin of susceptibility there.

We also have some preliminary weld data that indicates instabilities do not occur below approximately 200 degrees C. And I'll discuss that in the next slide.

We also have some alternative lines of evidence. What we have is aging times for the base metal indicate that degradation in the mechanical and corrosion properties do not appear likely below temperatures of about 300 degrees C.

We also have some natural analog information that indicates that the diffusion processes which result in these phase instabilities are very low at ambient conditions.

Josephinite, which is a natural metal mineral, has been stable in the environment for hundreds of millions of years. This material formed, at high temperatures, it formed a two-phase metallic structure that is unstable at room temperature, yet it has remained in this two-phase structure for millions of years, hundreds of millions of years.

This new data concerns Alloy 22 welds. Weld materials, or when you weld, you have about two volume percent of precipitates in the weld, in the as-welded condition. What we were trying to do here is see how much more, or what volume percent of these precipitates in the welds would increase susceptibility to chemical attack.

And so what we have plotted here is the aging time required to form both five volume percent precipitates and ten volume percent precipitates as a function of temperature. What we see in our work is that a five volume percent precipitates in the system, as judged by ASTM G28A, is the cutoff point for susceptibility to corrosion.

As an alternative line of evidence, we also have in the base metal looked at the effect on the chemical and mechanical properties of precipitation, as judged by Charpy and also the ASTM G28A. Again, what we have plotted is the time required to reach a certain amount of degradation of these materials as a function of time. Extrapolation to, and this should be years, not hours, indicates that for the base metal, times are less--the temperature to reach degradation and 10,000 years is greater than 300 degrees C.

On to general corrosion. What we considered in TSPA was that dry oxidation does not occur below a critical relative humidity; that aqueous-phase corrosion requires dripping water; that humid-air corrosion above a critical RH has the same rate as aqueous-phase corrosion; that the deliquescence point of sodium nitrate defines the critical RH for where we turn on humid-air corrosion; and that we assume that passive film was stable.

Continuing on with our TSPA results, the models were based on the weight loss data for Alloy 22, two year data, and for Titanium Grade 7, we had one year data from the long-term corrosion test facility.

The rates were independent of both temperature and water chemistry. We sampled the fraction of uncertainty and variability of the total variance, and that we also had enhancement factors for aging or precipitation of the unstable phases, and also we had an enhancement factor for microbially induced corrosion.

Changes that have occurred for SSPA. We now have a temperature dependent corrosion rate. What we did was assumed that the temperature dependency was based on the long-term corrosion test facility data for 60 degrees C. We assumed that the corrosion rate that we measured was occurring at 60 degrees C., and then we added a temperature dependence term, with an Arrhenius relationship. We determined the activation energy by potentiodynamic tests conducted at 80 to 95 degrees C., and came up with an activation energy of 36 kj/mol.

More recent data using potentiostatic tests covered over a wider temperature range from 25 to 80 degrees C., came up with similar activation energy for the corrosion rate.

We assumed that all variances from the weight loss data is due to uncertainty.

Now, this is just the experimental data which was used to calculate the activation energy for the temperature dependent corrosion rate. This data was all obtained from 5 molar lithium chloride solutions that had a certain amounts of chloride to sulfate ratio, varying from 10 to 1 chloride to sulfate, to 100 to 1 chloride to sulfate, over the pH range of about 2.7 to 7.8.

What this shows is the cumulative probability distribution function for the general corrosion rate for three temperatures, 25 degrees C., 60 degrees C., and 125 degrees C. What we see is using this method, that we now have an increase of about an order of magnitude between the corrosion rate at the median from 60 degrees C. to 125 degrees C., using our temperature dependent corrosion rate.

Passive film stability. As I mentioned previously, we have only considered that in TSPA, that passive film was stable. We have since instituted various experimental and theoretical efforts to understand this passive film stability under our conditions.

We have a generalized corrosion model that is based on the point defect model and the mixed potential model. These provide a mechanistic basis for extrapolation of our short-term corrosion data to longer times.

We have a localized corrosion model, the current model, which we will extend the current model to evaluate the breakdown of the passive film.

As a function of growth conditions, new studies are aimed at quantifying the growth rate of the passive film, characterizing the passive film structure, and evaluating any potential breakdown mechanisms. All three of these topics will be discussed later by Jerry Gordon this afternoon.

Just to briefly go into the point defect model, this model has been extensively developed for describing the growth and breakdown of passive films on numerous metals and alloy systems. It has been tested against experimental data and found to hold for a variety of alloy and environmental systems.

It provides an analytical relationship between the anodic partial current and the barrier layer thickness and potential. What it requires is a corrosion current as a function of electrochemical potential which corresponds to the passive current density. It also requires the identification of the principal crystallographic defects in the barrier oxide layer.

We obtained the corrosion current density from the mixed potential theories and feed that into the point defect model. But also the mixed potential model is based on the physical condition that charge conservation must be obeyed in the system. From this, we obtain a corrosion potential, E-corr, and also the corrosion current density.

These are some simulations that have been included in SSPA. We have two plots here, one is for the electrochemical potential versus oxygen partial pressure, and also corrosion current density versus partial pressure of oxygen. These are all for a stainless steel system, assuming pH of 3, saturated sodium chloride system, and also an electrolyte thickness of .01 centimeters.

We have theoretical data and we make a comparison against that for literature data for 316L at 95 degrees C. We see in both cases that the model over predicts the corrosion potential. And what we're trying to do is an experimental effort now to understand why it over predicts.

One of these that I mentioned previously was an understanding of the redox couples that are present in aqueous solutions. We want to understand which ones are being introduced by the environment, and which ones also may be introduced into the solution by degradation of the engineered barrier system components.

We also have an experimental study to evaluate the effects of these redox couples.

Localized corrosion considerations. In TSPA-SR, the localized corrosion threshold is based on corrosion potential and a potential as a function of contacting solution pH.

In TSPA-SR, we did not exceed the threshold potential, and so the localized corrosion model was never turned on. Now with our higher temperatures and somewhat different aqueous solutions, we feel the need that we need to evaluate this model more thoroughly, and so we're trying to get a mechanistic understanding of the localized corrosion process.

This includes the corrosion and threshold potential dependence both on solution composition and on temperature. And we also need to determine the Alloy 22 susceptibility over a wide composition electrochemical potential and also pH range. To this end, we're trying to evaluate Eh-pH diagrams, both experimentally and theoretically.

And then just to summarize, what I have here are a list of the various topic areas, how we presented them in

TSPA-SR, and also how they are presented now in SSPA.

Again, just to briefly go over some of the aspects, in terms of the environment, the range of chemistry that may contact the waste package, in TSPA, we considered that the solutions were mostly carbonate based brines. Now we're considering the possibility of near-neutral brine formation on the waste packages.

In terms of phase instability, the base metal, we considered it was stable. We now have theoretical modelling that supports this assumption that we made in TSPA. Long-range ordering was not expected below 260 degrees C. We now can extend that up to 300 degrees C. Welds were not considered. We now have experimental evidence that indicates that weld instability is probably not a problem under 200 degrees C.

For general corrosion, we had no temperature dependence. We now have a temperature dependence for general corrosion reactions.

Passive film stability, we had assumed that the passive films were stable in TSPA. Now we're undertaking a mechanistic modelling approach, and also trying to characterize them and characterize their kinetics of growth.

In terms of localized corrosion, we had always assumed that the threshold potential is not exceeded, therefore, we didn't turn on the localized corrosion model.

Now we are extending the studies to higher temperature aqueous solutions.

At this point, I'd like to turn it over to Joon Lee.

LEE: Okay, my name is Joon Lee. What I'm going to discuss is the remaining two other issues on stress corrosion cracking and early waste package failure. Then I'm going to focus on the effects of those quantified uncertainties of those parameters on waste package performance, mostly the effect of quantified uncertainty of stress corrosion cracking parameters, and also effects of Alloy 22 general corrosion models. Greg mentioned that we have a temperature dependent corrosion model. It has significant impact on the waste package performance. I will discuss this further later.

Then I will briefly summarize the updated SSPA waste package model. Then I will wrap up my presentation.

This is a list of the stress corrosion cracking model parameters in TSPA-SR. In TSPA-SR, we assumed that all pre-existing manufacturing flaws are oriented in such a way they can propagate radially in the presence of hoop stress. That is a conservative assumption.

The next one is all pre-existing manufacturing flaws are surface-breaking, and then they grow at the rate of general corrosion of the patch until the stress SCC initiates. That is a highly conservative assumption.

Then we have residual stress uncertainty bounds at plus or minus 30 percent of yield strength. And then we have a stress threshold for crack growth initiation set at 20 to 30 percent of yield strength. Then we used a slip dissolution model for our crack growth calculation.

Now, in SSPA, we have new data, new data for Alloy 22 in repository relevant conditions for stress threshold. So, based on that new data, we update the threshold stress to 80 to 90 percent of yield strength.

Let me leave this up here, and then let me go on. This is Alloy 22 constant load stress corrosion initiation test results. This test is ongoing, and the test is continued at concentrated J-13 well water and at 105 degrees C. As you see here, the Alloy 22 specimens have varying conditions, such as crevice specimens and welds.

And then the specimens are subject to the stress, stresses ranging from somewhere between 170 percent of yield strength, all the way up to 20 to 50 percent, not shown here, but some of the specimens were subject to 20 to 50 percent of yield strengths. And up to 2500 hours, none of the Alloy 22 specimens show any sign of stress corrosion crack initiation. So this is hard data for Alloy 22 in Yucca Mountain relevant exposure conditions.

Then we have additional data from the long-term testing facility at Livermore for U-bend samples, and none of those U-bend samples show any sign of stress corrosion crack initiation.

So based on this data, and additional data at the long-term testing facility, we updated the threshold stress for stress corrosion crack initiation to 80 to 90 percent. But if you look at the data here, still 80 to 90 percent of yield strength is highly conservative.

Let me go here. The next is stress corrosion cracking parameters. We evaluated in SSPA the residual stress uncertainty bounds in the outer and inner closure-lid welds. This evaluation is based on literature data mostly, data with similar stress mitigation techniques. For outer closure-lid welds, we are going to use induction annealing to mitigate stress. Based on the literature data, we updated the stress uncertainty bounds to plus or minus 21 percent of yield strength.

For inner closure-lid welds, which will be mitigated using laser peening technique, we used the data for similar mitigation techniques, which is sharp peening actually, and we used the data to evaluate and update the stress uncertainty bounds. In SSPA the stress uncertainty bound is sampled from a distribution with a median of plus or minus 15 per cent and an upper bound of plus or minus 30 percent.

The left-hand graph shows a stress uncertainty bound from 5, 10, 30 percent, and with 80 percent of yield strength. This is the lower bound of updated threshold stress of 80 to 90 percent. In TSPA-SR, as I mentioned earlier, we used a 20 to 30 percent of yield strength as the threshold stress, about this line here. So what it shows is that in SSPA, we need to have a deeper penetration before stress corrosion cracking can initiate basically, you know, a few more millimeters of additional depth.

The right-hand graph is the stress uncertainty profile for inner lid weld regions. What you see here for inner lid closure weld regions is that 80 per cent of yield strength is about the bound of plus or minus 30 per cent. What is means is for inner closure-lid welds, there is a very small probability we have stress corrosion cracking. I will discuss this further in terms of waste package performance. It has a significant impact on waste package performance. So only a very small number of waste packages have stress corrosion cracking failure.

Other parameters that we looked at and evaluated in SSPA are repassivation potential in the slip dissolution model. The update of this value is also based on recent Alloy 22 data.

The next one is orientation of manufacturing flaws. We looked at additional literature data. Also we used the limited measurement data from TSPA-VA mockup, and we used additional testing to characterize flaws in closure-lid welds of TSPA mockups. And based on this additional data, we updated flaw orientation, and that updated model has lognormal distribution with a median of 1 percent, and then plus or minus 3 standard deviation bounds at 50 percent and .02 percent. I will show you the impact of this updated uncertainty on the waste package performance.

Now, moving on to early failure of waste packages. In TSPA-SR, we did an extensive literature survey for potential mechanisms for early failure. All identified mechanisms were screened out based on low probability arguments, except weld flaws. Weld flaw is used as input to stress corrosion cracking modelling. That was in the TSPA-SR model.

We evaluated the information available for those potential mechanisms and we identified that improper heat treatment, mostly for induction annealing of closure-lid welds, and that the probability of improper heat treatment is estimated to be 2.23 times 10 to minus 5. This information is already documented in project AMR, tied to the early failure of waste package AMR.

This probability actually includes the probabilities for non-detected equipment malfunctioning, also non-reported operator errors. This information is actually from the handbook.

For a total of 11,770 waste packages, and the waste package number is from the TSPA document, we expect that the expected number of improperly heat treated waste packages is .263. At this point, we used this as a mean probability. Then we applied a Poisson distribution to estimate the number of waste packages in the repository. So this new analysis indicates that 20 out of 100 realizations have at least one waste package affected by improper heat treatment. Three realizations have at least two waste packages that failed early. That is the status of improper heat treatment of waste package failure in SSPA.

I want to point out, you know, kind of a conservative assumption we used in SSPA with early waste package failure. We assumed that affected waste packages, one or two waste packages, fail immediately when corrosion initiates. I think that later today, Mike Wilson shows the history of waste packages, with early failed waste packages. And we assume that conservatively, weld regions of both the outer and inner closure-lids fail immediately, even though, you know, inner closure-lid has some distance there, we assumed that for affected waste packages, both lids fail immediately. Again, we are not taking credit for the stainless steel inner shell.

Now I move to the waste package performance analysis to evaluate the effect of those quantified uncertainties. This is the TSPA waste package analysis results. I want to spend a little time on this result.

In TSPA, we present the waste package analysis result in summary statistics based on 100 realizations. And this left-hand red curve is the upper bound. This is the 100 percentile curve. And this curve is the lower bound. The upper bound has extremely low probability associated with that. When you look at the upper bound, we are looking at the extreme of the extreme.

And then I also want to point out that the upper bound has a very low probability, and that usually that the 95th percentile curve represents a reasonably conservative case. And for the TSPA-SR base case, the earliest possible first breach time, which is the extreme of the extreme, is about 11,000 years, and the first breach time of the 95th percentile, which is reasonably conservative representation, is about 21,000 years.

This is the waste package analysis result for the update models, the stress corrosion cracking parameters. The black curve is the TSPA-SR base case. And then the green curve is for the case using updated residual stress bounds, and the blue one is the case using updated flaw orientation, and the red curve is the case using updated stress threshold.

As you see, this case, you know, we performed this analysis by changing one parameter at a time, keeping other parameters the same as in the TSPA-SR model. By adding more, we improved the model. For each component, we have improvement on waste package performance. As I mentioned earlier, with updated stress threshold, we have significant improvement of waste package performance.

The next one is the effect of quantified uncertainty in Alloy 22 general corrosion model parameters. Greg covered those parameters in detail, so I'm not going to go into detail. But let me jump to the waste package results.

Again, this is the 95th percentile waste package failure histories. And, again, this black curve is the TSPA baseline model results, and this blue one is assuming the case using 100 percent uncertainty in the total failures of Alloy 22 corrosion rate. The red curve is the case using the temperature-dependent general corrosion model.

As you see, the case with the temperature-dependent general corrosion model has a significant impact on waste package performance. Why? There's two things. One is in the repository, most of the time, the packages stay at low temperatures. The temperatures are decreasing with time continuously. So the packages stay in high temperature only a very short time, and most of the time in low temperatures. So with time, the waste package general corrosion rate is decreasing continuously. That gives much better performance of waste packages. The second thing is using this temperature-dependent corrosion model, we are modelling, representing variability of temporal spatial variability, because the waste package temperature is changing temporally and spatially in the repository.

This is a list of model components. They are uncertainties that are updated and quantified in SSPA. We already covered this one.

This is a comparison of the waste package performance results using updated SSPA model and TSPA results. This is the result using updated SSPA, and this is the TSPA base case. I'm showing only upper bounds and 95th percentile and mean. As you recall, the waste package performance using the updated stress threshold and temperature-dependent Alloy 22 general corrosion model, you see that it is more like additive, so that's why you see here, the upper bound of the SSPA model has delayed the first breach time to beyond 100,000 years.

To summarize my presentation, new data and analyses have been developed to quantify the uncertainties in corrosion model parameters. Also, we improved the technical basis for the performance of waste packages. And, in SSPA, we included the early waste package failure due to improper heat treatment.

And as I mentioned several times, waste package performance is significantly improved from the updated stress threshold and temperature-dependent Alloy 22 general corrosion rate.

And then I want to point out we have significant margins, safety margins, in the waste package performance in the TSPA-SR base case model.

That's all I have.

SAGÜÉS: Thank you very much. We may have some questions here. And at this time, I'm the first, and then we have Dr. Craig and Dr. Bullen.

Let me get started. I think that I would like to see, since you're standing, but I have some questions for Dr. Gdowski, I would like to understand a little bit more about the improper heat treatment issue, because I understand how you may have a faulty weld, and I understand that you may have a failure to detect a flaw in a weld, but what is the meaning of improper heat treatment? Are those defects caused by improper heat treatment, or are those defects, think there were pre-existing that the heat treatment didn't quite eliminate?

LEE: That the technical basis for this one is not really related to defects. It's more like in doing the heat treatment, for example induction annealing, that there could be a probability that we could have heated that closure weld to a higher temperature than it's supposed to be, or we can heat the weld longer than it's supposed to be. That, as Greg presented in his aging and phase instability issue, that could cause substantial aging problems. So the aged Alloy 22 can lose constraints and corrosion resistance. So we assumed those, the packages could fail immediately when corrosion initiates. It's a conservative assumption, yes.

SAGÜÉS: Okay. So the heat treatment itself introduces the problem?

LEE: Right.

SAGÜÉS: Okay, very good. Thank you.

The other question I had was on Slide Number 16. Okay. And I'm concentrating right now on the green curve which would be the TSPA-SR mean. Right? Is that correct?

LEE: Yes.

SAGÜÉS: And the corresponding curve in SSPA would be the right-most curve over there.

LEE: Right.

SAGÜÉS: That is about an order of magnitude improvement from one to the other.

LEE: Uh-huh.

SAGÜÉS: Can you tell how much of that improvement is due to the assumption of temperature variability of uniform corrosion rates, and how much is due to better behavior in resisting corrosion cracking?

LEE: Okay.

SAGÜÉS: Because those are the two main factors that cause the shift. Is that right?

LEE: Yes, the information is there. Would you put Slide 12 up? For update to the stress threshold, if you look at, and this is the 95th percentile case, by the way, the first breach time in the TSPA-SR model is about 20,000 years. And then for SSPA, using the stress threshold, it's about 40,000 years. So in terms of the first breach time, it gives about an additional 20,000 years.

SAGÜÉS: Okay. So that's a factor of about three changes, about a half an order of magnitude?

LEE: Right. Right. Would you go to the next one please? Slide 14, okay. Now, the effect of--improved the temperature-dependent general corrosion model is about 50,000 years. So, we are looking at 30 to 40 additional time. So, to improve the stress threshold, we are looking at 20,000 additional years, and about 40,000 additional years. If you combine this, about 60 to 70 additional years. If you add that to the base case, we are looking at about 100,000 years additional time.

SAGÜÉS: Okay. So then the temperature dependence change seems to account for somewhat more than the stress corrosion cracking.

LEE: Yes, that's correct.

SAGÜÉS: But not like one is ten times greater than the other, and so on.

LEE: It in fact has a multiple effect, because with a slow general corrosion rate, it will have more time for stress corrosion cracking initiation. It gives additional time. But once we reach that, we have a high threshold stress, so kind of a combination of both.

SAGÜÉS: Okay, very good. I have one more question for Dr. Lee, and I have a couple of questions for Dr. Gdowski.

If we go to Number 5 in your presentation, I just wanted to point out something to make sure that we all understand what we're talking about. And you presented information which represents a very long experiment here, it's a couple of months, or three months long kind of exposure, and so on, and you obtained an important piece of information, that after 2,300 hours, there is no deterioration propagating under those conditions, and so on.

Now, just to underscore the nature of the extrapolations that we're talking about, you have there about 1 meter graph from zero to the 2,500, or so. If we go to 10,000 years, that graph--I made a quick calculation here--it would take us about some 30 miles in that direction--maybe a hundred miles, or something like that, but seriously, as an engineer and not a scientist, what can you say about applying this kind of information to 10,000 years time frame?

LEE: My professional judgment is that--Greg and Jerry

Gordon are over there. They can, you know, add some more. But the one main reason we are looking into passive stability is exactly that reason. We want to do a better handle on the long-term stability of passive film in more like a mechanistic deterministic modelling, using the known science and principles. And then our model is not that mature yet. It's ongoing. So once we have that model fully developed for use as a long-term prediction, then we can combine that modelling with a--testing data, we combine that, we should be able to present more convincing--I understand that that is the issue in here, you know, long-term predictions.

So, I don't know if Jerry or Greg wants to add some more.

GORDON: If you look at the yield stress line on there, and you go down to 80 to 90 percent of that, there's quite a spread between the actual data and the 80 to 90 percent.

In addition, if you remember the crack growth data where you deliberately initiate a stress corrosion crack, you essentially have to keep cycling it at a high stress intensity to keep it growing. And as soon as you go to a static load, the crack tends over time to turn off, and the waste package is probably as static a stress state as you can get. So it's really that combination that gives us confidence in raising the threshold.

SAGÜÉS: Okay, very good. Quickly, I would like to ask a couple of questions for Dr. Gdowski, if I may.

One of the things I wanted to ask you was about the metallurgical developments after long terms of annealing at low temperatures in Alloy 22. And you showed a couple of transparencies on that, and I think that we can go to, for example, Number 6 in your presentation.

And, again, here we have another issue of extremely long-term extrapolation. They developed there in a few weeks, a couple of months, kind of information, and what you are doing there is what all of us do in our respective--we just slap an Arrhenius relationship on that, and we extrapolate that, and we can estimate an activation energy, and so on and so on. But as you know very well, that involves a string of assumptions, and one of them is, okay, we're going to have first of all one activation state. If we have another activation state at the lower level, that may not kick in until later, and you end up with different--and then you can get even more fundamental, you know, most of these things assume--usually the distribution or some such assumption. So there is a lot of baggage behind just taking that line and feeding it to some kind of an Arrhenius relationship.

How critical of an approach has been taken in--through the process that leads you to drawing that line?

GDOWSKI: This is preliminary data. I agree with you.

What we have instituted is both a long-term aging study similar to what we have for the base metal, so that we're going to do it at much longer times, and over a range of temperatures also. And we've also instituted a modelling program similar to what we have done for the base metal, where we're trying to understand segregation that occurs in the welds, and the phase instabilities in that region, trying to model on a molecular level, if you will, what is going on in the welds to give us confidence in this extrapolation. Very similar to what we did for the base metal.

SAGÜÉS: And that's work that is in progress?

GDOWSKI: It's in progress right now, yes.

SAGÜÉS: And how long do you think it's going to take to continue with that work, and to get it to the point where if other members of the technical community or the public would ask the same question, say, a few months from now or a couple of years from now, how close would you be to answering that question at that time?

GDOWSKI: I think we'll gain more confidence as time goes on. Obviously, as we go to longer and longer times, we can decrease the amount of extrapolation that we have to do.

I think one thing that we do know is based on some of the natural analogues, like the Josephinite, where we had high temperature metastable phases that have been, for hundreds of millions of years at low temperatures, and we don't see these sort of phase instabilities occurring under those conditions, even though after they were formed, they were at high temperatures for extremely long times, hundreds to thousands of years.

SAGÜÉS: And they're still there?

GDOWSKI: And they're still there, yes.

SAGÜÉS: Very good. Thank you very much. And in the interest of time, I will hand it to Paul Craig.

CRAIG: Okay. Paul Craig, Board.

Alberto has gone in a direction I really wanted to go in to some extent, having to do with fundamental mechanisms. It's my personal belief that if you're going to extrapolate over three or four orders of magnitude, to do so without a very strong fundamental mechanism is simply not convincing. That's not a direction, however, I want to explore right now.

Greg noted that localized corrosion is now beginning to play a role. And in preparation for coming to this meeting, I read the most recent material that I have, which is apparently not the most relevant material because things change. I referred to something called SSPA, the Supplemental Science and Performance Analysis, dated June 2001, which seems fairly recent, and so my question is based on that, even though I now realize that it's out of date.

In that volume, and in the Supplement Draft

Environmental Impact Statement, we learned that the peak dose in the first 10,000 years is zero, precisely zero, with no uncertainty bounds. That apparently has now changed a little bit. But as of June 2001, it was correct, according to that document.

So I went into the document to try to understand the kinds of things that are driving that conclusion, and I looked in particular at localized corrosion assumptions. Now, a really important point is that if you have something that's 2 centimeters thick and the corrosion rate is 1 micro per year, that material will do fine for 10,000 years. But if the corrosion rate is increased by only a factor of two, to 2 microns per year, then it will not do so well. So factors of two really do matter. If they happen to be in the exponent as in an activation analysis, they matter more. But these are simply straightforward factors of two.

So I went into Page 7-7 of the document I just referred to, the SSPA, where I look at Alloy 22 localized corrosion rates in C-22, and what I discovered is that for C-22, a log uniform distribution of rates is assumed. And for titanium, a uniform rate distribution is assumed. Which immediately leaves one to wonder why is one distribution log uniform and the other is uniform, and what is the theoretical justification for that.

But that's not the point that I want to ask about.

The point that I want to ask about, and we can't do everything now, the point that I actually do want to focus on is the end point. And for the C-22, the end point--remember, this is a rectangular distribution, which has a very well defined lower and upper bound--the upper end is given as 1.27. For titanium, the upper bound is given as 1.12. That's three significant figures by way of thinking. So this is a distribution that marches along, and then absolutely cuts off.

Now, what I fail to understand--and, remember, that this is in a region where a factor of two can make the difference between working extremely well and failing completely. So, if I take that 1.27 and I multiply it by two, I'm up to 2.54. And if we think about this in terms of microns per year, that takes us from a region, which happens not to be the units here, but never mind that, that takes us from a region where it work, to a region where it doesn't work.

So, the area that I want to get at here is that the conclusions that you are drawing are enormously influenced by assumptions about the nature of the distribution and by the fact that you have an extremely sharp cutoff when you use a uniform distribution, and you give us every reason to believe that you believe that you know those cutoff points, one, exist and, two, that you know the three significant figures.

And I look at all of that and I say this is simply not convincing. Now, why am I wrong? Why should I be convinced?

LEE: Let me try to respond briefly, and then I'll hand it to someone over there, Greg or Pasu.

The first thing is--the first thing about what about the corrosion is a factor of two higher or a factor of two lower. That's exactly why we are doing the--in WAPDEG. We try to capture those uncertain ranges, if you will, for corrosion rate. That's why presenting the waste package result--this I'm not sure is an answer, but the second one, how do I put that--I'm not the author of that AMR, you know, so I cannot speak very well for him, but Joe Farmer is no longer with the project. But my understanding, and Pasu or Jerry can add later, that rate, if you look at the reference, the rate, actually the rate is from highly, highly--he, I don't know, he tried to capture or include all the data available in the world for Alloy 22.

Now, going back to more realistic Yucca Mountain conditions, I think we have to look at the rate distribution again to be more representative of conditions we have. That's my short answer. But Pasu or Jerry or--

CRAIG: But clearly to the extent that local corrosion never cuts in in the model, the point that I'm raising is irrelevant. But on the other hand, if localized corrosion does begin to appear, then the question does become more relevant. I simply picked out localized corrosion because the handle, as an example, even though it wasn't included in that particular model, as I understand it, say the June 2001 CD that I was looking at, now it is included, but the same kind of question can just as well be asked with respect to other types of corrosion.

If you're going to have a rectangular distribution with a well defined end point, that is going to heavily influence the conclusions that you draw. And you seem to be enormously confident about these end points, as given by the three significant figures that are presented there. And it's this general treatment of uncertainty issue that I'm trying to get at. There's many places where the same kind of problem arises.

LEE: Another point I want to point out is that Yucca Mountain, I said that the repository in light of--what I meant by saying that is is that in the Yucca Mountain condition, we have potentially beneficial anions, like carbonate, potentially beneficial anions in the water, carbonates such as sulfate and nitrate. Those rates, according to the table of the AMR, does not--not representing that kind of--I think Greg can speak to it better than me. But the issue related to localized corrosion, I think Greg and others are also looking at further--if that happens, what is the consequence, mostly the effect of the presence of beneficial anions. That is not done completely.

CRAIG: So you're telling me that you're giving me three significant figures, but the area is not relevant to Yucca Mountain, is that right, because the environment is completely different?

LEE: Right.

SAGÜÉS: Okay, I'm going to go ahead and let Dr. Bullen--

CRAIG: Greg, maybe you can explain this to me later on.

BULLEN: Bullen, Board.

Actually, I just have a couple questions, because my esteem colleagues covered a lot of this stuff that I was interested in.

I'd like to go first to Joon's Figure 16. And maybe this is a precursor to a question that will be answered this afternoon. But yesterday afternoon, we saw Peter Swift's presentation about the new dose calculations, and I asked a question if you'd reached the peak yet. And if you take a look at that far right curve on Figure 16, you'll see that we're talking about the SSPA mean for the failure distribution of the waste packages. And so if you carry that extrapolation out, I'm assuming that it's going to look sort of similar to the shape of the curve for the TSPA mean, so we're going to end up with about 100 percent of the waste packages failed in a couple of million years, ballpark number?

LEE: Yeah.

BULLEN: So, will that type of extrapolation be done to make a look at a peak dose? And maybe I'm looking at Bob Andrews now, and I probably should ask maybe Mike Wilson this afternoon as a more appropriate one. I know you cut it off at a million years, but it looks to me like only about 62 1/2 percent of the waste packages have failed by the time you got done. So if you wanted to look at peak dose, wouldn't you want to carry that all the way out?

And Bob is shaking his head yes. Do you want to go on the record and say that that would be what you would do? Not that you're promising to do anything more by, you know, the day after tomorrow or anything. But it just seems to me that that's the answer to the question I asked yesterday. Am I missing something?

ANDREWS: No, you're not missing anything. You're right. It was unclear from the previous figure whether you would actually achieve the peak at a million years.

BULLEN: right.

ANDREWS: So you would want to confirm whether that was the peak, or whether the peak was somewhere out at two million years or seven million years, or whatever.

BULLEN: And I guess the question I'm asking here is is there anything else that I'm missing here? It looks like the waste package is what's driving the peak that far out, and so that you would expect that distribution to be shifted because of the waste package performance.

ANDREWS: That's correct.

BULLEN: That's good. Now, I'll defer to other questions later, but I'd like to go to Greg Gdowski's Number 11 slide. These are the data from Scully at the University of Virginia.

GDOWSKI: That's correct.

BULLEN: And I notice, you know, you always have to explain the outliers, and the outlier there, you know, looks kind of significant if you want to take a look at a temperature dependency, because if you look at those inverted triangles and included the outlier, you get a significantly different slope, which is a different activation energy, which gives you a much greater temperature effect. Is that not true?

GDOWSKI: That is true.

BULLEN: So you're being conservative here by throwing the outlier out by saying that the temperature effect is actually somewhat less; is that right?

GDOWSKI: That's right. You would have a higher rate at higher temperatures, but the rate at lower temperatures would be decreased significantly.

BULLEN: Right. And so I guess the other question that I have is that it looks like it's quite a wide range of conditions. I mean, besides the factor of ten in the buffering of the lithium chloride solution, you also have the pH range of 2.75 to 7.75?

GDOWSKI: That's correct.

BULLEN: If you maybe unfold the data, do you see some buffering effects? Do you see some pH effects? And the reason I'm asking this question is that, as Alberto pointed out, these data sort of make a significant reason for a shift of, you know, a half an order of magnitude, or so, in the performance of the waste packages. So I just wondered if there's further work being done, and will we see, you know, more results, and if so, when and what do you expect to see?

GDOWSKI: Further work is being done. I don't know if there was any trend in the data with pH or with the sulfate and chloride ratio. It's something that we plan on investigating more thoroughly, looking at both nitrate and sulfate and their inhibiting effect on the corrosion process, and doing that potentially of cyclic polarization type testing for that very reason, to see what sort of quantity of sulfate and chloride we need to bring about an inhibiting effect. Those tests are ongoing. I don't know if we had enough information from there to make conclusive statements about that with this data at this time.

BULLEN: Thank you. I guess the only other point I'd like to make before I close is that I am very interested in that outlier, and also the fact that there may be a more significant temperature effect than you're currently taking credit for, which means hot is worse than cold, I guess is the bottom line statement.

GDOWSKI: Outlier is of interest to us all, so we're trying to understand why it's there.

LEE: May I add one more? We are very certain this is outlier because, you know, if we use this, as you mentioned, we have slopes like this.

BULLEN: Right.

LEE: If we extrapolate the slope to higher temperature, we have like a corrosion rate of, you know, corresponding to carbon steel. We know that Alloy 22 is not like carbon steel. If that is lower temperature, we have a sudden--in the corrosion rate. So we know that this is actually the outlier.

BULLEN: Bullen, Board.

I guess you're right about those extrapolations. But the thing that I'm interested in is there a different mechanism that's kicking in, and does it really have an effect that occurs somewhere around the boiling point? And if you've got that kind of happening, I mean not that it turns into carbon steel and alka selzer and goes away real quickly. But there really may be a significant temperature effect. And if there is a significant temperature effect, it would be nice to know that, you know, you're a whole lot better off at 80 degrees C than you are at 95 degrees C.

GDOWSKI: And one of the things that we're also doing is doing autoclave studies so that we can extend it much above the boiling point, so that we can see that temperature effect that you're talking about.

BULLEN: Thank you.

SAGÜÉS: We still have a bit of interest. Some other Board members, Christensen, would like also to ask a question. We also had a question from a member of the audience, and I'm sure--but unfortunately, we do have a schedule that we have to adhere to. So I will thank the presenters very much, and go ahead for the next presentation, which is by Pat Brady on waste form process components.

BRADY: All right, I'm going to spend about the next 20 minutes describing the waste form process components. This represents work that was done under the direction of Christine Stockman. You're going to see some overlaps, though, with the EBS components that Bob MacKinnon talked about yesterday.

What I'll do is I'll provide an overview of the various calculations that we perform in the waste form group, and then I'll focus on the unquantified uncertainties that we spent a great deal of effort on in the past four months. These are the in-package chemistry calculation. Specifically, the way that we estimate the range of pH is likely to exist inside the breached waste form. I'll describe new models for predicting the dissolved levels of Neptunium and Plutonium in those fluids.

I'll briefly touch on EBS calculations for in-package transport. In particular, I'll outline the new model describing in-package diffusion, and our treatment of in-package sorption. I'll show you some updated cladding calculations. I'll summarize it all by considering the low temperature implications, and there's some special twists that affect waste form that don't affect the others there. And then I'll conclude by talking about other lines of evidence.

I should point out this represents work done by a lot of people other than myself. Yueting Chen, Eric Siegman, Paul Domski, Mike Gross and Jim Schrieber.

Now, there's five primary calculations that we perform. The first one is the in-package chemistry calculation. Now, what we do is we consider a breached waste package as a constant volume reactor. That is, we take a seepage flux, put it into a waste package, react it with the components inside, steel, glass, fuels, and what have you, and them have it drop out, have the water go out.

I should point out right here that we're considering breach after the thermal pulse has passed. Our calculations begin by assuming we were talking about 20,000 to 40,000 years after the repository was closed. That's changed somewhat, as Joon Lee mentioned early on. But for the first part, we're talking about low temperatures.

There are a lot of uncertainties that go into this calculation. First of all, the absolute volume of seepage going in is not something we can identify within an order of magnitude, so we use a range from 15 liters per year per waste package, down to .15 liters per year. We use a range of clad exposures from the clad being 1 percent exposed, to 100 percent exposed. We fix the partial pressure of oxygen equal to the ambient atmospheric. We fix the partial pressure of CO2 to ten to the negative 3.0.

One of our other primary inputs is the thermodynamic database out of Livermore, which I'll talk about in greater detail down the road.

So we put in our inputs. In addition to this, our inputs are degradation rates of the steels and the fuels. We put these into the reaction path model, and our ultimate objective is to calculate a pH as a function of time inside the waste package.

Now, once we do that, we want to be able to map onto that trajectory the dissolved concentrations of neptunium, plutonium, uranium, technetium and other radionuclides of interest.

So while we do this calculation, there are independent calculations being done to establish what these surfaces look like. In particular, we use EQ3, an equilibrium speciation code, to develop a pH dependent solubility curve for each of these things. Note that most of the solubility limiting phases for most of these are oxides, whose stability depends on pH. So we take those functions, map them onto these pH trajectories to estimate absolute levels of these over time.

Now, the EBS people are doing another calculations where they estimate the impact of in-package sorption. Note that when we try to get a handle and comprehend what the absolute levels of these things are in the waste package, there will be both dissolution and re-precipitation, which we account for here. But natural systems are often affected by sorption as well. To assess the impact of sorption in our system, we have to estimate the masses of sorbing phases and their affinity for these specific radionuclides.

There's an in-package diffusion calculation I'll talk about in a bit. Clad integrity is calculated in parallel with a semi-independent model.

Let me show you first of all what one of these pH trajectories looks like. If you look at the boxes here, this is one of our base cases for a commercial spent nuclear fuel run. We start off using J-13, and the pH inside the waste form starts up at an optimum level, and then it drops, gets down below a pH of 5 at around 200 years. This is due to the dissolution of the A516 steel, in particular, due to the oxidation of the sulfur component.

Once that is exhausted and the steel is gone, pHs start to climb again due to the influx of J-13 water. There's another drop out around, let's see, 20,000, 30,000 years, and that's due to a longer term expression of 316 stainless steel dissolving. In particular, it's the oxidation of the chrome in the steel, the accumulation of chromate in the waste package fluids that causes this pit.

Now, one of the first uncertainties that we've had to consider is, well, what is the composition of the seepage? The upshot here is that the composition within certain bounds does not matter a great deal. We've used J-13 here. We've used J-13 that's been evaporated up here. We've used water from unsaturated zone waters, and what you see is after just a few years, the waters all take on the same signature. In other words, these waters become intimately impacted by the dissolution of the waste package components. So, this is one we crossed off our list of uncertainties that we have to quantify.

Let me make one final point on this slide. We have several types of waste packages that we have to consider at the site. Most of them are going to be commercial spent nuclear fuel. There's going to be a fraction that also contain glass. The high-level waste glass has a profound impact upon the pH trajectories. When you dissolve glass, pHs go alkaline fairly quickly, fairly quickly in a geologic sense.

This is what the co-disposal package trajectories look like. There's the initial pitting, the initial drop in pH, followed by a rise to pH 8 and 9, sometimes higher. And that jump right there comes directly from the dissolution of the glass.

I should point out that when we look at the co-disposal packages, there's lots of them, we've used the fast flux test facility and the Fermi fuel as our co-disposals.

Well, the primary uncertainties we see in the in-package chemistry calculation are the degradation rates of the steels. What I've shown here in this somewhat confusing slide is that when we vary the steel degradation rates, we can move the trajectories back and forth.

Now, at the insistence of the NRC, we've done a number of sensitivity calculations where we've changed the degradation rates, we've varied the fluid flow rates to see just how robust is this pH range that we seem to reside in, about 4 to 8 for the commercial spent nuclear fuel, and around 4 to 10 for the co-disposal. And what you'll see in the next slide is the results of the sensitivity test.

The point of this slide is to show that failure analysis has allowed us to make some improvements from the S&ER results. Specifically, if we look at single trajectories, we can split them into fairly precise regions of pH behavior, and these we can abstract out for feeding into the TSPA.

Okay, these are our sensitivity runs. One of the concerns that we've had to consider is what happens with fluids that come into the waste package and only interact with--well, they don't interact with everything. We do the calculation by, in effect, taking the whole waste package, grinding it up and titrating it into water, or rather putting water into it, homogeneous waste package.

We might expect that in real life, water is going to see very unique pathways, and so we did sensitivity calculations where we would remove one component and see how it changes the pH trajectory. The upshot of this slide is that the only real deviations that we get from our normal range, the only big excursions occur when we pull the 516 steel out, or when we pull out the glass. That's one of these that's going up here. Glass free; 516 free. Those are the two that really are the only outliers.

So that's sort of the snapshot of the in-package chemistry calculation and what we feel the bounds are. Now, remember that once we have the pH trajectories, we want to map on solubility functions.

This shows the original treatment of neptunium dissolved concentrations. It was based on the solubility of Np 205. Note that this would, if we use that line to predict the amount of neptunium in solution in the waste package, we would have some very high levels. Note also that those high levels are almost never seen when we do, for example, drip tests, or tested at PNL or drip tests at Argonne.

So, what we've done to establish a new function, is we have looked--we've tried to get a more mechanistic basis on the neptunium dissolved concentration estimate, and we rely on the observation that neptunium seems to be released from the uraneal phases stoichiometrically with uraneal. In other words, there's a one to one correlation, or rather, the amount of neptunium that comes off is directly proportionate to the amount of neptunium in the solid phase.

So, we can take a solid solution model for neptunium in the uraneal phases and we come up with this line right here. Specifically, this line is calculated from the neptunium levels that we can predict with origin, constrained by the uranium release dictated by schoepitesolubilities. Schoepitesolubility is pH dependent, hence, this line is also.

The green bands represent the three sigma uncertainty. These dots right here, I should point out, were not--this model wasn't calibrated with these dots. These dots are Argonne National Laboratory drip tests.

So, the upshot here is we've got, when we go from the Np 205 model to the solid solution model, we have a lower mean, but we have--the mechanistic explanation is better. We're hoping to improve this as we're still doing experiments.

For plutonium, this was the range that we had in the S&ER. What you see up here are--well, this is the new expression we have, the new mapping device for plutonium. And it comes from letting the dissolved plutonium levels be set by two phases at two redox states. We consider--well, this line like here, if you look at the dots underneath the line, that is the solubility of PuOH4 amorphous at ambient O2.

Now, inside the waste package with all of the--it's going to be surrounded by steels that are corroding, and we might have pooling water. We might expect to have lower redox states. and this right here is PuOH4. PUOH4 is less soluble under lower FO2s. This is PuOH4 at--its solubility is a function of pH--this is 40 log units lower than ambient. That's kind of a ballpark from looking at natural waters that we come up with.

This line right here is PuO_2 crystalline, the solubility calculated as a function of pH at ambient FO_2. This is at the lower FO_2. Again, the more crystalline PUO_2 has lower values than the less crystalline one.

So, our model now we considered an improvement over the other one because we can link this line right here to specific mineral species. These dots in here, this clump right here, are values from the PNL batch tests. These over here, those are plutonium concentrations measured at the Argonne drip test.

We're also doing work with John Haschke on looking at the stability of plutonium super-oxide as well.

All right, I'm running out of time, so I'll make these next ones fairly fast. The in-package transport calculation was briefly described yesterday by Bob MacKinnon. This involves estimating the amount of water that's sorbed on the breakdown products inside the waste package. You estimate effective diffusion coefficients using Archie's Law, and calculate diffusion fluxes through various paths of the waste package. According to Bob, these things--diffusion outside of the waste package can delay release by roughly a couple thousand years.

All right, this one I've had more contact with. Things like uranium, their transport in the environment is typically limited by dissolution and re-precipitation. Trace elements, their transport tends to be controlled by sorption. Now, inside the waste package, there's no shortage of sorbing phases. There's been 15 and 21,000 kilograms of iron oxide and iron oxyhydroxides per waste package. There's also about 10 kilograms of copper.

The reason I mention this is that the Kd's for the sorption coefficient for things like neptunium and plutonium--well, they're very high on iron oxides. Copper oxides and copper sulfides are one of the few minerals that have a pronounced tendency to sorb iodide and pertechnetate. They have high anion exchange capacities, and the Kd's for iodide and pertechnetate on copper oxides and copper sulfides go anywhere from 100 to 3,000.

The Kd's for neptunium sorption onto the iron oxides, as I recall, it's somewhere between 5 and 500. Multiply that by at least a factor of ten, and you have the Kd's for plutonium on the same phases.

Mike Wilson is going to talk about the impact that this has on repository performance later on this afternoon.

The calculation of clad integrity is a function of time. The two most important ones here are the second one and the second to last one. The distributions have been changed on the creep and stress corrosion cracking perforation from a triangular function described by those parameters, to a uniform one described by those.

In a nutshell, what happened was we went from a rate derived from un-irradiated cladding to one derived from irradiated cladding.

Rock overburden failure, this was left out early on. Now, there's a function that gets rock overburden failure, which is proportional to waste package corrosion.

Okay, I've got two slides left here. Low temperature implications. As I said before, our calculations were done at low temperature because we were dealing with post-thermal pulse efforts. Now we're starting to worry about early failures, and so we're considering what happens with this whole infrastructure once you go up temperature. This is exactly opposite what everybody else is doing. They start at high temperature and went down. We're going the other way. And we're only about knee deep into this right now, but there are a number of semi-qualitative evidences that suggest that the effect of temperature is not going to be that profound for us.

First of all, a number of the radionuclides we care about have retrograde solubilities. Surfeit is one of them, and since the neptunium concentrations are linked to surfeit, it ends up with a retrograde solubility. We're doing experiments to test that, though.

Higher temperatures have lower gas solubilities. In other words, all other things being equal, a high temperature fluid has less oxygen and less carbonate. That tends to stabilize a number of the radionuclides.

The corrosion rates, Joon and Greg talked about earlier, I'm not going to get into that here.

Temperature affects the in-package diffusion calculation by affecting the condensation of water, as well as the calculation of the effective diffusion coefficient.

In-package sorption is--well, there just aren't a whole lot of experiments that have been done looking at the effect of temperature on sorption. Those that have been done suggest that sorption increases as you go up temperature.

Cladding, according to Eric Siegman, who is the man in charge here, anything below 350 degrees keeps us where we want to be. There is a weak temperature dependence inside of the clad unzipping rate. I think it's linked to the spent fuel oxidation.

The other lines of evidence. We have lots of them. I should point out, though, that our other lines of evidence, in the in-package chemistry calculation, our other lines of evidence are used to support the rate laws we used, our primary inputs, the thermodynamic data.

The glass degradation rates, we've used a range, and we've considered high values measured in the lab, to very low values measured in the field. Geologically, we tend to observe glass in the field, the rates drop over time. The longer you measure them, the lower they get, and we get these--this picture comes from looking at the rate that silica comes off of volcanic glass on volcanic edifices, and what have you.

Steel degradation, this is again Joon and Greg covered this earlier. Thermodynamic data, since a lot of this--this is somewhat indirect evidence. But to the extent that many of our low temperature thermodynamic data come from solubility measurements, or experimental measurements, there's some I guess you could call it another line of evidence. A more persuasive and compelling one might be the other lines of evidence that we can marshal for neptunium and plutonium, and we have--I showed you the drip test and the neptunium concentration.

In the neptunium slide and the plutonium slide, those values were not used to calibrate the model, but they were instead found to be consistent with the model.

Other lines of evidence for in-package sorption. Well, the sorption Kd's that we have came from compilations of sorption measured in the field, and measured at hazardous waste sites, mine sites and what have you.

I should point out, though, that one of the things we're looking at is something that pops out of the other lines of evidence is when you do sequential extractions, for example on bomb pulse plutonium in soils, you find that a lot of it is irreversibly sorbed. That is, it will not--according to a Kd model, which tends to--what it says is that as we move towards more reliance on these things, we're going to see that some of these things stay more put than we might otherwise predict.

Cladding. Eric Siegman assures me there's an extensive experimental literature attesting to the features of his model.

Now, to summarize, I'll hit the highlights here. What we've done in the past four months is we've come up with a better understanding of the coupled features that control the bound of pHs in the in-package chemistry model. We have a new neptunium secondary phase model. We've got a more anatomically correct plutonium model. We've updated the clad model.

We've calculated in-package diffusion for the very first time, and we have started to explicitly include sorption in the in-package chemistry in EBS.

With that, I'll close.

SAGÜÉS: Thank you very much. We have a question from Dr. Bullen.

BULLEN: Bullen, Board.

Actually, I've got about three questions. But if we could go back to, say, Slide 4, I kind of wanted to get a better understanding of what I'm looking at here. As I see the dip in pH, you mentioned that it's driven by the fact that the A516 is the buffering agent, or the agent that drives it down. So is it correct to assume that it takes like 40 or 50 years and all the A516 washed out of the waste package? Is that what I'm looking at?

BRADY: It's a couple hundred.

BULLEN: Couple hundred?

BRADY: Yeah. We start off using--to be conservative, we use very high rates.

BULLEN: Okay. So, it's a couple hundred years. Then, I guess if I go to Slide 5 which is the next one, there's kind of a band there that looks like it's somewhere between--you know, starting to see the washout at about, I don't know, 50 years going up to 200 years, in that range?

BRADY: It's right there, yeah.

BULLEN: Okay. So, does that apply that it would be better if there was more A516 in the package? More sorption on the ion oxyhydroxides and all that stuff, is that the indication that you can draw?

BRADY: No, I wouldn't--let me pause to try to understand what you're saying. Okay. So, the--

BULLEN: I guess, I'm just trying to understand why the pH changes; pH changes because the iron goes away, right?

BRADY: Yes.

BULLEN: Okay. So, if there was more iron, it would take longer for the pH to come back up or--

BRADY: Let me back up here. Okay. The pH gets driven down here because the A516 is pumping sulfuric acid in. It gets to that point, it's all gone.

BULLEN: Okay.

BRADY: And, it gets diluted as you go back up. So, I don't know if I've answered your question.

BULLEN: Okay. Well, I guess, that leads into the next question because what are the uncertainty bands on this or does that represent the uncertainty bands as you go from pH4 to pH8? That's kind of as high as you get or are there plus and minus a couple of sigma? Where would you put the plus or minus couple of sigma on there if you were doing it?

BRADY: Well, the answer is yes. This is the uncertainty band, but the last time I checked with the TSPA, if you would--let me back. We don't use these lines, we abstract them by putting line segments through them and we tend to put an extra dip of pH on the lower end and to the higher end and you can actually see it here for the co-disposal ones. What you do on the--it has a low value and a high value. What's been done for the abstraction band is the minimum, you average the minimums of all of the runs, and the maximum, you average the maximum for all the runs. So, there's somewhat of an implicit spilling over to account for the uncertainty.

BULLEN: Okay. Can we move on to Figure 8, please? You were describing essentially the plutonium dissolution rates and right at the end, you said you were doing a little bit of work on super oxides. So, am I to assume that that means the hyperstochiometric oxides of plutonium that you're going to end up with when you get to really high burnup fuel or maybe a mixed oxide fuel? Is that a good analogy or--

BRADY: No, it's when you have the PuO_2+X that has received some attention. There is now an article by--

BULLEN: Right, that's the hyperstochiometric?

BRADY: Yeah. And, we're kind of at the front end of that right now. So, I can't tell you how it's going to work out.

BULLEN: Okay. But, you are addressing the issue and so things like MOX fuel and high burnup fuel which may end up being an issue with respect to the amount of plutonium that you have and how it might dissolve are things you're going to address?

BRADY: Yes. I'm sorry, I see your question. Yes, you are right.

BULLEN: Okay, good. And, then, the last question I have goes to Figure 11. In this case, you talked about the two important factors, one of them being the second one, creep and stress corrosion cracking and you changed the distribution from a triangular to a uniform distribution with a half a percent as the top of the uniform distribution based on new data. And so, you're saying that it's the data from a radiated and an unradiated clad. And, I guess, is that also drawing upon the data from the test are north, information that came out of the dry storage environment? There's some information there basically that the clad performed pretty well for, you know, 15 or so years. I was wondering if there's some data from Einzinger or one of those guys up there that basically was drawn into this determination of the change in uniform distribution?

BRADY: My eyes are gazing across the crowd to see if Eric is here to field that one.

BULLEN: That's a little bit detailed. Maybe, we can talk about that one off line. That's just one of the areas that I'm very interested in because cladding performance, although not necessarily explicitly in there, is one of those backup for safety. So, I'd just like to know how you came up with these numbers. I'm interested in the difference between the radiated and unradiated clad, but I can do that one off line.

BRADY: I know we cited Einzinger because I can remember his name from filling out the reference list.

BULLEN: Well, I just know there's new data that came out of test area north that you should look at.

BRADY: Okay.

SAGÜÉS: I see no questions from other Board members. But, I understand that Dr. Diodato and Dr. Di Bella have questions. And, we have a couple of minutes. Dr. Diodato?

DIODATO: A very interesting presentation. On Figure 8, if we could look again at Slide 8 where the plutonium stability phase is? As you know or may have been questioned, the Board has definitely expressed an interest in the DOE demonstrating and understanding the fundamental processes in a number of different areas. So, when I saw these dots on this curve, on the various curves, lining right up with the curves, I got excited, but then I realized that probably those were your speciation calculations. Is that correct?

BRADY: Yeah, I'm sorry, the last two phrases, I didn't catch.

DIODATO: The dots on the curves, you have a speciation calculation, for example for PuO_2 crystalline and going down and then going right across the line in the middle there and the upper curve for $PuOH_4$, those are theoretical?

BRADY: Oh, yeah. Yes.

DIODATO: Yeah, that's based on speciation calculations.

BRADY: Right.

DIODATO: So, it's nice to have a speciation model that can predict things and believe in it, but then if I look at the Argonne drip test, open circles around pH6, then it seems to me that--

BRADY: Right.

DIODATO: You get like a six order of magnitude spread there in the experimental data. So, that's somewhat discouraging maybe if you're not used to that sort of spread. But, that's just kind of an observation. The real question was Bob MacKinnon yesterday presented analyses that suggested pHs might be down around 5 or less, you know, more acid pHs for a period of, say, 3,000, 4,000 years or so. And, it's my impression that secondary uranium phases might be of significant value in terms of prediction of what might happen in terms of potential repository. What's your understanding--still, I guess in an oxidizing Eh setting, what's your understanding of the stability of the secondary mineral phases and how that lower pH might affect those phases with time in those early times?

BRADY: Well, let's see. That's kind of an open-ended question. Let me deal with, first of all, the assertion that--the question about these things. Yeah, these are (inaudible) calculations, these are drip tests, and there's a six order of magnitude spread. I should point out that I was kind of constrained for time, but the Argonne drip test, the PNL batch test are not exactly analogous to the input calculations you do when you sweep across and do the solubility. So, these things provide indirect support. They are not, you know, something we pull amounts from. Now, your question about what happens as you go lower, things become less stable. We'd like to stay away from the low pHs. And, that's why we invested a fair bit of time and effort in looking at just how realistic the low pH spikes were because again they're directly proportionate to how fast you choose a steel rate today. I don't know if I've addressed your concern, though.

DIODATO: Pardon?

BRADY: I don't know if I've answered your question, though.

DIODATO: Oh, yeah, thank you.

BRADY: You know, in natural solutions, it's hard to maintain pHs much below 4. You just don't observe these things unless, you know, they're special conditions, you know, effluent from a mine.

SAGÜÉS: Dr. Di Bella?

DI BELLA: Carl Di Bella, Board staff. Two questions, a quick one and one a little bit longer. The quick one is on Overhead 13. The second bullet says steel degradation, persistence of reduced iron at Yucca Mountain. I'm sorry, I'm just not making that connection of what you're referring to. If there is reduced iron at Yucca Mountain that persists, isn't that really an anti-analog, something that contradicts equilibrium assumptions that are apparently being made in the calculations? Can you tell me more about what's being referred to here?

BRADY: Okay. The argument was that there's a persistence of magnetite and you can argue that--and, Carl, my response when someone said it was about the same as yours, it's, oh, yeah, that's because it's carried with hematite or ferric hydrate or something like that. But, if one tries to envision a waste package filled with degraded waste package stuff, it's very easy to imagine a lot of the radionuclide-bearing solids being intimately in contact with reduced iron in contact with that iron oxyhydroxide coating. So, the question becomes is it more reasonable to pick an oxygen fugacity of the earth's surface or is it more reasonable to pick one down around hematite and magnetite? The value I used of 10^{-40} is halfway between 2, right? Now, this is an uncertainty that we're going to continue to have to address because it is notoriously difficult to identify what the actual redox state is in natural waters. It has a propound effect on the solubilities there.

DI BELLA: And, Overhead 6, please? Thank you. I was struck on this by the fact that of the whole series of slides you had of pH versus time that the lowest pH that was on this slide was 3. It seems to me that with all of the iron and nickel, chromium, and so forth that there would be a large possibility for hydrolysis occurring which would result in lower pHs than that. So, there must be some sort of assumption of some sort of naturalization agent coming in and reacting with this. Can you explain why we don't see hydrolysis apparently?

BRADY: There are reactions that tend to buffer the--well, let me back up. There are finite limits to the amount of acid which can be produced and they are defined by the amount of, in this case, the A516 steel. Now, the buffering reactions that mitigate against it down here, it's glass dissolution, but also these things right here, you end up with a lot of uranium, uranium--the solutions are very concentrated and you--the solutions have their own buffer capacity and what also happens to keep things together--what stops things from going well-below 2 is dissolution of iron oxides that form in the first place. We're calculating that you get roughly 10 moles of either ferric hydrate or FU_2O_3 per mole of water that goes in--excuse me, per liter. Now, if you go to pH2, you're going to dissolve all of that iron. If you write the reaction to FU_2O_3, it goes to FUOH plus. You end up conserving protons. So, the degradation products themselves anchor the pHs. It's not much of an anchor--I mean, you know, anchoring a pH at 3 and that's still an acid pH, but you can't get down to 2 unless you dissolve a large fraction of the iron.

DI BELLA: Thank you.

SAGÜÉS: Thank you very much.

We're standing between a crowd of people and their coffee. So, we better break right now and we'll reconvene at 10:01.

(Whereupon, a brief recess was taken.)

SAGÜÉS: We're now ready to continue with the rest of the session.

We have three more presentations. The first presentation will be by Jim Houseworth with contributions by a substantial number of collaborators and Jim is from Lawrence Berkeley National Laboratory working for Bechtel SAIC. He's going to be talking on unsaturated zone transport process components. So, please, let's take our seats and listen to Dr. Houseworth's presentation.

HOUSEWORTH: I would like to go over now the recent progress in unsaturated zone transport primarily documented in the SSPA Volume 1 and list the number of people who directly contributed to this effort.

This is the set of topics I'm going to be covering. The first items will be conservatism in radionuclide transport, trying to get a handle on the conservatisms that we have identified in process and PA models for radionuclide transport. The first item will be drift shadow which concerns the subject of radionuclide transport in the vicinity of waste emplacement drifts and then we'll go onto radionuclide transport calculation methods which focus on both process and PA models, focuses on the effects of fracture-matrix interaction. Then, I'll go into thermal operating modes. Accompanying the same set of topics that Bo Bodvarsson covered yesterday in terms of the mountain-scale, coupled processes, and expansion of the repository footprint, and I'll be addressing these in terms of effects on transport. Then, I'll finish with multiple lines of evidence.

Okay. I'd start off with the drift shadow. I have a conceptual drawing of the process in the vicinity of a waste emplacement drift. We've done quite a bit of modeling and field testing in terms of drift seepage and over the years have found that there's a substantial amount of diversion of that seepage. The percolation flux is diverted around the drift. The effects of that diversion have immediate impact in terms of the environment of the waste emplacement drift and those effects have been captured in PA and process models. The effects in terms of transport through the drift are that you have diffusion and limited releases from the drift because you have no seepage under those drifts. The number of drifts that have been found to have complete seepage diversion are quite high in the PA model. In the Rev 0 calculations, we have found an average of about 87 percent of the drifts didn't seep. That number has dropped in some of the more recent calculations, but it still a large fraction, on the order of half the drifts see no seepage over long time periods.

The consequences of that seepage diversion continue below the drift. And, that aspect hasn't been captured in process models or PA models up to this time. The consequences are caused by this flow. This diagram really represents what's happening in the fractures in terms of this strong gravity-driven flow pattern along the sides of the drift and down below the drift leaving a zone of very low-fracture saturation, very low, poor velocities in the fractures. In terms of the matrix, the capillary forces are much stronger. Water can be pulled back more quickly to meet the drift and we see much less of this kind of flow pattern beneath the drift in terms of the matrix. So, what you get is a dry fracture zone, but the matrix only has limited effect in terms of reduced saturation of the flow. In terms of transport, you have, as I mentioned, the diffusion limited transport out of the drift because there's no flow going through the drift. What those radionuclides see at the bottom of the drift is rock where fractures are near-residual saturation, very low saturations. Matrix is somewhat depressed, but it's about 1000 times more water content in the matrix than in the fractures. That results in diffusion finding pathways in the matrix in that portion, but most of the release is into the matrix. Then, the subsequent transport is affected by the slow diffusion and advection in the matrix and only gradually returning to fractures as it moves down below the drift.

I wanted to comment on a couple other aspects of this problem. Bo showed yesterday the thermal drift shadow, so to speak. In that case, we saw an enhancement of this saturation reduction below the drift driven by coupling with thermal processes. But, the aspect that I'm going to talk about today in terms of the quantitative results are for a non-thermally perturbed system and so it will exist as long as the drift exists and diverts seepage.

Another aspect that I won't be able to cover because we didn't do any calculations, but we think is important, is the effects of this on colloid releases. Colloids also have to diffuse out in this environment and it's well-known colloid diffusion is much lower than diffusion of aqueous species. The colloids will also have a much more difficult time exiting the drift and getting into the rock matrix because of the size exclusion effects that are expected. Therefore, we think the colloids in this environment will have a very difficult time ever leaving the drift.

And, I'm going to discuss some calculations that were done on this grid that's shown here just to give you an idea of the scale. It's a subsystem model that only encompasses the tsw35 unit. It's 65 meters in depth and 40 meters in width. Here's the drift here, a 5 meter diameter drift. We take advantage of some of the symmetry conditions along the drift to center line, and along the center line to the pillar for doing the calculation.

Next slide. Here are some of the transport results for this subsystem model. Bottom line is that transport in the drift shadow is also much longer radionuclide transport times than in the baseline transport models, not surprisingly when I tell you of how the baseline transport models work. Let's go over that first. In the baseline transport model, the releases go directly into fracture flow that is undisturbed by the presence of the drift. So, what we find is, going through this subsystem, 45 meters for the drift. The radionuclides get there in about a year. That's for direct release into the fracture flow. In the drift shadow model where we take advantage of the fact that the flow is much redacted beneath the drift and the radionuclides have to diffuse into the matrix and start their transport in the matrix, you get much longer transport times. Here, we show a couple of cases where there's been sensitivity studies done here with different flow rates in the matrix, 1.6mm/yr flowing through the matrix and .3mm/yr flowing through the matrix, which represents our estimates of the range of possible matrix flow rates. The overall percolation rate of

10mm/yr. So, the remainder of the flux is moving to the fractures. Here, we see 3500 years to 6200 years of transport. For neptunium which is a weakly sorbing radionuclide, we also see much longer transport times and, in fact, these are enhanced by sorption out to 11,000 or 12,000 years in this case.

In terms of our TSPA implementation, we've only been able to partially implement this work in the TSPA. We've used what's called an advective-diffusive flux splitting algorithm. In this case where we have advective releases in the drift, those enter the flowing fractures that are again undisturbed by the presence of the drift. Where we have diffusive releases from the drift, we put those into the flowing matrix which is also undisturbed by the presence of the drift. So, these aspects of the flow field in the drift shadow model haven't been incorporated into the TSPA.

There is also another aspect that is potentially non-conservative that's in the TSPA abstraction and that is for radionuclides that do enter the matrix. In this abstraction, they cannot diffuse back from the fracture. They have to either advect back into the fractures or simply transport out of the bottom of the system in the matrix. We think that that probably only affects the long-term behavior in the PA; things like peak dose. The other aspects which are conservative are more important to the initial delays that we show in the TSPA model.

Next slide, please. Okay. Now, jumping subjects here to the radionuclide transport calculation methods, the baseline process models that we use for radionuclide transport are dual-permeability models, both for flow and transport. Here, we show a schematic diagram of dual-permeability model where we have fracture connections going down here. This is a one-dimensional model and matrix connections like this. You see, there's only a single grid--or two grids, basically, between fractures and matrix that can be used to represent the exchange between these two continua. Because the models are based on spatial gradients of state variables, such as pressure and concentration between these two grid points, we only get a certain level of refinement in those gradients which is limited by the (inaudible). To investigate the effects of this, we implemented what's called the multiple interacting continuum models which is a big name just for putting more points in the matrix so we can get a better gradient and try to capture more precisely the effects of these gradients on the exchange between fracture and matrix. This is a little diagram which shows what was used. You have the vertical fractures here showing the planes and then a series of nested zone inside the fractures which represent the matrix and shows how we can get a better handle on those gradients between fractures and the matrix. So, we implement this into flow and transport models in a two-dimensional site-scale calculation. What we find is that the DKM model predicts much earlier breakthrough in the initial phases. The explanation is pretty simple. When you initially have these fronts coming through, the concentration gradients are sharp and we're unable to capture those with the two point method. When you break this into a multiple point kind of approximation to the gradient, you get a better representation and it does draw things into the matrix more quickly and slows down the transport.

Well, that's the study we did in terms of the process models. We also have done some investigation of some of the conservatisms that are represented in the PA transport model which uses a different calculation scheme. PA model implemented a continuous analytical representation of the fracture-matrix exchange, specifically to address the issue I just discussed in terms of the dual-permeability versus the multiple interacting continuum method. But, in order to do that, you had to introduce some other approximations that affect fracture-matrix exchange. When we compared the PA model which is FEHM, Version 2.1, against an alternative transport model that more directly implements the dual-permeability conceptual model, what we found was more conservative breakthrough curves. Here's a plot. This is for a 3D site-scale transport calculation. For technetium, you see earlier breakthrough for the PA transport model than process model, and similarly, even greater discrepancy for a weakly sorbing radionuclide, neptunium. We are working on trying to implement a transport method that is more uniformly valid, more realistic, but hasn't been yet implemented in the PA at this time.

Next slide, please. Okay. Now, going on to thermal operating modes issues. The first aspect that we addressed was this expansion of the repository footprint in order to allow for a lower thermal operating mode. What we show here in this red outline is the baseline repository block and then the blue line shows a slight expansion and this region added to the south to give more emplacement area for low thermal operating conditions. The flow transport calculations have been done on this footprint and what we have over here are the simulated breakthrough curves for technetium and neptunium in this 3D site-scale transport model. If you look at the north block versus the south block for technetium, there's the south block, what you find is that there's a fair amount of slower breakthrough from the south block. That's primarily driven by reduced infiltration and somewhat longer distance between the potential repository and the water table than in the north. But, when we integrate the two which is this dashed line and compare it with the baseline TSPA-SR calculation, the results aren't terrifically different, basically, because this is a smaller emplacement zone. So, in total, the effects are small. And, similar results were found for neptunium.

In terms of thermal operating modes and the thermally-driven coupled processes, we've looked at several of the different coupled processes in terms of their effects on flow with some quantitative analysis that Bo presented yesterday. I'm not going to go over those again. We didn't do any quantitative analyses of these in terms of transport, but we did use these to guide qualitative evaluation of the effects of these coupled processes on transport. What we found for the mountain-scale TH that the effects of these thermally-driven processes on thermal-hydrological processes gave fluxes in the high-temperature case that varied over a range that was very similar to the range of changes that we would see in the climate change without any thermal degradation. The lower temperature operating case, the ranges or variation in flux below the repository were even less than that. So, basically, TH processes are expected to have a limited influence on mountain-scale radionuclide transport and have been, therefore, not propagated into the TSPA.

Drift-scale is a little different because of the more pronounced effects of TH processes on the drift-scale. High-temperature operating under local dryout of fractures and matrix in the vicinity of the drift, and in fact, it's a longer dryout below the drift, on the order of 2500 to 3000 years and will prevent transport until rock rewetting occurs. In the low-temperature operating mode, there is local dryout in the fractures, not in the matrix, that would likewise prevent releases to the fractures for on the order of 2000 years. Matrix water is retained. So, things like diffusive releases in the drift shadow model would go on, but this reduction in fracture saturation would also--oh, okay. The overall effects of this drift-scale dryout was not included in TSPA, but may result in improved performance if there are releases during the thermal period.

Next slide, please. The effects of thermal-hydrologic-chemical effects were looked at in terms of their effects on mountain-scale flow. This was done in a two-dimensional site-scale model and found that variations caused by precipitation dissolution events in the fractures caused variations in permeability less than one order of magnitude. This is smaller than the natural variability in the fracture permeability, and therefore, we expect that these processes would have a limited influence on transport and flow. Because these were negligible, it again wasn't propagated into the TSPA. For THM effects, similar bottom line. We did look at a mountain-scale and drift-scale THM model and the variations in fracture permeability driven by THM processes ranged from about 10 to 40 percent; again, much smaller than the natural variability of the fracture permeability and excluded from TSPA on that basis.

Next slide, please. In terms of lines of evidence, fracture-matrix interaction is a key element in the transport model, both for the baseline transport model and for drift shadow effects, as well. I might comment that in terms of fracture-matrix interaction, in the baseline model it's always conservative to reduce that interaction because things moving through the fractures can only be slowed up by fracture-matrix interaction. In the case of the drift shadow model, that relationship is inverted. The things that are entering the matrix can only reach fast transport pathways through fracture-matrix interaction; therefore, reducing that in the drift shadow model improves performance. It's the opposite effect.

So, what are the observations we have? Well, probably the best observation is that hydrologic observations of saturation and water potential in the matrix remains unsaturated despite a relatively large percolation flux through the mountain relevant to the permeability of those matrix units. That is used in the flow model calibration to set the fracture-matrix interaction. So, it's not exactly an independent line, but it's certainly a very important observation. It's one that we have been able to use quantitatively. We also have observations of geochemical disequilibrium in the system. We haven't been able to take samples or observe fracture water, per se, except in perched water bodies, and there, we do find disequilibrium in the chemistry between the fractured water and the matrix pore water.

Another indication of reduced fracture-matrix interaction is the steep gradients found in uranium between fractures and matrix at the Nopal I unsaturated zone site, Pena Blanca. This is again an indication of reduced fracture-matrix interaction which is a component of those baseline and the drift shadow model.

In terms of long transport times, we have a couple of observations which suggest transport times may, in fact, be longer than what we calculate in the Yucca Mountain system. The lower mobility of uranium in the unsaturated zone at Pena Blanca indicates that the uranium has not moved substantially over 100,000 year time frame, even though there are indications of water events over shorter time periods. Similarly, at the Akrotiri anthropological analogue site in Greece, there has been found limited migration of copper and lead over a 3600 year time frame in the unsaturated zone.

Next slide. To summarize, finally, the transport times in the drift shadow are considerably orders of magnitude longer than predicted by the existing PA model, at least in the subsystem that we investigated. Transport times in the drift shadow are significant relative to the 10,000 year regulatory time frame. Again, that's in the subsystem. In terms of colloids, we expect that they would have a substantial impact on the release of colloids.

Process model representations of matrix diffusion have been shown to be conservative. That's both in the representations we've used for the process models and comparisons between the PA model and the process model. Including the southern extension to the potential repository block, that has resulted in slightly longer transport times to the water table.

In terms of thermally-driven coupled processes, the only one that seems to be of significance is the local dryout which would cause delay of radionuclide transport immediately beneath potential waste emplacement drifts during that period of dryout. The other thermally-driven coupled process effects are expected to have minimal influence on transport.

That's the end.

SAGÜÉS: Okay. Thank you very much. Do we have any Board questions? Dr. Craig?

CRAIG: Paul Craig, Board. It was very interesting. An observation and a question. The observation is that on Figure 6, the technetium transport, half the water got down to 50 percent breakthrough. It used to be 300 years and it's now moved up to roughly 800 years which is certainly an improvement, but it's still a time short in comparison with the regulatory compliance. So, that's very interesting and encouraging, but still you're under 1,000 years for that.

The question is the following. Back in the PA transport model, you had a different model than we have in the drift shadow model and I'm trying to understand the difference. And, my reference point for all of this is a calculation that Bo taught me about years ago done by Phillips which was a closed performance solution for homogeneous medium that clearly showed the shadow effect in that instance.

HOUSEWORTH: Uh-huh.

CRAIG: And, now, in the finite difference model taking into account the fractures, I would expect that the same kind of phenomenon would emerge automatically naturally because you're solving essentially the same equations.

HOUSEWORTH: That's correct.

CRAIG: And, the question that I don't understand, as yet, my question to you, is why didn't everything that you're seeing now show up in those earlier calculations? What has changed between then and now that's caused new effects to appear?

HOUSEWORTH: Well, we didn't do the drift-scale flow calculation that would capture that in previous calculations.

The drift-scale calculations that we did do were limited to what's affecting what's coming into the drift.

CRAIG: Okay. So, how did you--

HOUSEWORTH: We didn't really continue or use any of those finer scale flow calculations that would go on below the drift and certainly never looked at them in terms of transport.

CRAIG: So, you're saying that just below the drift, you had a--how was it handled? You had a very large mesh that--

HOUSEWORTH: Yeah. As far as the way it's handled, we have--

CRAIG: Was handled.

HOUSEWORTH: Yeah, was handled. Is that there is a large site-scale grid with grid blocks 100 meters by 100 meters. And, the releases from the drifts simply go into those grids and the flow fields in those grids have no idea that there's drifts there. There's no drifts in the model.

CRAIG: Okay. So, it's smaller mesh than they did in those?

HOUSEWORTH: Right.

CRAIG: Thank you.

BULLEN: Bullen, Board. Actually, I'd like to take a look first at one of your backup slides. That's the hazard you have of putting backup slides in there. Could we go to 13? I guess, the question that I have is that, you know, this is a distribution of the flow of the drift shadow effect and you mentioned some percolation fluxes in the matrix and in the fractures that are on the order of 10 for the fractures and 1.3, 1.6 for the matrix, kind of, mm/yr.

HOUSEWORTH: Right.

BULLEN: What happens in the changing climate when you've got pluvial conditions? How do you expect the shape of these curves to change? Does the drift shadow effect become less significant with more water flowing around the top of the drift?

HOUSEWORTH: I have another backup slide that can help on that--I mean, better than this one--if you'd like to see that.

BULLEN: Okay. Go for it.

HOUSEWORTH: In fact, there's two, 15 and 16. We did a case where we had 100mm/yr going through the system instead of 10. And, here's the comparison for technetium. I don't have the values out here, but we're still out in the thousands of years, a couple thousand years. It does come back a bit. The reason is that with the increased fracture flow, you have some higher fracture saturations, and therefore, your fracture-matrix interaction is higher. But, you still get pretty long transport time in the 45 meters.

BULLEN: Okay. Then, I guess--

HOUSEWORTH: And, the next slide shows it for neptunium.

BULLEN: The last question that I have goes back to Slide 3. As you look at the drift shadow zone that's there, this is a highly idealized matrix and grid layout. And, say, we have the hot operating mode and we were actually looking at water shedding between the drifts. Now, the assumption is the water shed is pretty much straight down, but I'm assuming that the fractures and the faulting isn't necessarily up and down. So, you may have some lateral diversion in the fractures.

HOUSEWORTH: That's correct.

BULLEN: And, you picked 45 meters straight down in this member of the Topopah Springs tuff.

HOUSEWORTH: Right.

BULLEN: What's the probability that, indeed, I don't have that fracture zone extending quite that far, but I actually have water flushing from the column that's essentially draining and coming across and maybe washing through my drift shadow and sort of negating the effect. I that something that you've considered?

HOUSEWORTH: No, we haven't looked at a thermal perturbed system. Now, I would ask you what's driving the flow laterally? Because we do have the forces involved. Now, we don't have heterogeneity and we do intend to look at heterogeneity.

BULLEN: Oh, you just answered my question.

Heterogeneity is what's driving--

HOUSEWORTH: Okay. Okay.

BULLEN: I've got fractures that don't go straight up and down and, I mean, in reality--

HOUSEWORTH: Right, right. In reality, I would expect those could as easily--the heterogeneity of the system could easily divert as cross-convergence. It's a point. I mean, it needs to be investigated, for sure.

BULLEN: Yeah, I mean, if there's a possibility that it could influence the performance of this and you're claiming thousands of years of improvement, you've really got to have some justification for it.

HOUSEWORTH: Right. Well, it hasn't been developed too much. That's kind of a new thing for us. We haven't gotten that far.

BULLEN: Okay. Thank you.

PARIZEK: Parizek, Board. A couple of slides that you were showing like this one, 3, and then also Backup Slides 13, 14. For the model runs you do, how long does it take to see evidence of a drift shadow forming? The idea here, if you're going to go look for this in the field, how old a tunnel do you have to have in order to have any hope of finding it because one suggestion is why not go into some of your niches and drill on the floor and take a look? But, the model runs took how long to get the kind of differences you've shown on, say, Slide 13 and 14?

HOUSEWORTH: Well, I was really running these to steady state, as you're aware of. But, the actual time for that, I don't recall.

PARIZEK: You could probably--I mean, we could dig it out of your--

HOUSEWORTH: Yeah.

PARIZEK: I mean, what, five years or eight years, whether you should use the ESF or cross-drift or something like that especially when you need to go to some older excavations?

HOUSEWORTH: I think it would be relatively quick for the fractures which is the important effect. The matrix would probably take a very long time, but since--so, I think that we could expect to see some--Bo, did you have something?

BODVARSSON: This is Bo Bodvarsson of the Lawrence Berkeley Lab. A couple of comments. Number one, with respect to Paul Craig's question about the groundwater travel time in that picture saying it increased from 300 to 800 years, I just wanted to point out we are doing our realistic case AMR which addresses that issue once more and the groundwater travel time both in SZ and UZ exceeds 1,000 years certainly. So, you will see that when that comes out in October. These calculations, these show the (inaudible) for groundwater travel time purposes, even though they were just comparisons.

With respect to Dan Bullen's questions about the shadow zone and the possibility that heterogeneities may cause water to destroy our beautiful shadow zone, I want to say the following. When you put waste in place and you dry out the rocks and the factors around it, we haven't done the full calculation with heat, but we get 1000 years of delay just from the heat alone. And, knowing from our seepage calculations that we can't even get waters to get into the drifts from above in the higher temperatures tuff because it all boils off, it will certainly all boil off in a higher temperature case below the drift, too. So, the advantage of the higher temperature case for the shadow zone are unquestionable. It's just the basic physics and the physical processes of the rocks.

Finally, I think Jim was exactly on the right track with regard to the time frame of developing a shadow zone. The fracture drainage underneath an opening is something that takes just a few years to develop, not hundreds of years and certainly not thousands of years. Like he pointed out, the matrix changes may take longer, but they are immaterial because the saturations in the matrix doesn't change what's in the shadow zone nor does it have to because that's not really the effect. The fact of the matter is the waste goes into the matrix and then diffuses is the important part. So, we think with tunnels that have been excavated for some tens of years, like the G-tunnels or something like that, would be potential candidates for drilling into them from the floor down.

PARIZEK: Parizek, Board, if I can continue, you bring up the point on colloids. I've been interested in this whole idea of colloid transport through the unsaturated zone. The evidence for it or lack of evidence for it is based on the difficulties from some of the field experiments. But, if it's a good floater and you get trapped in the bottom of the footprint reading of the emplacement drifts, that's a lot of colloids. So, eventually, in time, you can imagine a lot of the fracture openings becoming plugged, and if water continues to come in someday in the future, will we get ponding or puddling? So, have you looked at the idea of the colloid buildup in that interval?

HOUSEWORTH: No, we haven't, but I'd point out that the case I was talking about was a case where the drift didn't allow seepage. So, there wasn't any flow into the drift.

PARIZEK: Yeah. But, somewhere during the pluvials, you're going to have seepage somewhere, maybe?

HOUSEWORTH: Yeah. Well, from the Rev O calculations, they found 87 percent of the drifts didn't seep for 100,000 years that they were--

PARIZEK: So, they should only--

HOUSEWORTH: Now, in the more recent calculations, I understand it's dropped to about 50 percent using different focusing factors, using future climates that are a little different. I'm not sure in that case whether or not that necessarily precludes flow for such long periods of time into the drift, but that case may come up in this situation for that you mentioned. But, we haven't looked at that, no.

PARIZEK: Yeah. The point is that there's a really huge amount of colloid material that's going to come out of the waste packages and from the waste package themselves. So, it all accumulates on the floor. Some portions of the floor should--may cause ponding. That's the point.

The other observation is that the pH data we had from the previous Speaker Brady, for instance, yesterday showing the low pHs that could come out of waste packages in the early stages of their degradation also makes it an acidic environment and transport. And, in your transport calculations, do you assume acidic environments or what do you do with pH for transport?

HOUSEWORTH: Well, no, we assume, more or less, ambient condition environmental chemistry. For technetium, there's no sorption assuming it continues to transport as a (inaudible) anion. Under those acidic conditions, there wouldn't be any effect. In terms of neptunium, there could be some effects on sorption of the acidic environment.

PARIZEK: What it seems to do to me is raise the value of the Pena Blanca analogue study because of the low pH environment there with the sulfite and minerals that are oxidizing or been oxidizing so that that analogue study may take on greater significance as a source term analogy in terms of what happens under a lower pH environment.

SAGÜÉS: We're going to have to cut this conversation because we have two more presentations this morning. Thank you very much.

We're going to here now a presentation by Bruce Robinson of BSC/Los Alamos all on the saturated zone process components.

ROBINSON: Yes, it is Los Alamos despite the title slide here. I'd like to acknowledge the other folks who participate in the saturated zone. I'm going to be reporting on work that's gone on and documented in the SSPA. People from Sandia National Laboratories work with us at Los Alamos and the USGS to put together the body of work that I'll be reporting on today.

The topics I'm going to be discussing are new data and model analyses that have come out since the Rev O, the TSPA/SR. I'll then go into unquantified uncertainties analyses. We don't work looking at certain distributions of parameters and looked at the impact of changing those distributions based on new data. I'll spend some time on multiple lines of evidence, confirmatory lines of evidence for various aspects of the saturated zone model, and then I'll conclude with an analysis based on the newly published 40 CFR Part 197 standard and what impact that will have on the calculations in the performance of the saturated zone.

First, the new data. The Nye County early warning drilling program has provided us the opportunity to collect a variety of different types of data; lithology data in the wells, as well as hydrologic information such as head and water level elevations. I'll show you a little bit of that. The wells also give the opportunity to perform testing.

The next topic is testing at the alluvial testing complex. We have hydrologic measurements and also preliminary data from single-well tracer tests which we believe provide us information on the conceptual model for transport in saturated zone.

Then, I'll talk about some new model analyses. We're expanding the scope of the modeling to include alternate conceptual models for various features that we see in the groundwater system beneath Yucca Mountain, as well as the role of the Solitario Canyon Fault. I'll also show a sensitivity analysis for a larger repository footprint which would happen as a result of a cooler repository design. And also in the documentation, but I won't have time for today, are some additional dispersion and matrix diffusion analyses.

First, the new data. The lithologic and hydrostratigraphic information that we're gaining from the Nye County wells consists primarily of a better characterization of the extent of the alluvium. If you recall, the conceptual model for transport in the saturated zone consists of fracture flow and matrix diffusion in the volcanic rocks transitioning to a porous medium flow in the alluvium. However, since these wells are dots on a map at this point, we have a dearth of data in this general region which doesn't allow us to really pin down the transition, where that transition from volcanics to alluvium occurs, while this data has started to allow us to get a handle on that. Well 2-D which is right here at Highway 95 showed greater than 800 feet of saturated alluvium. That is starting from the water table. And, 19-D showed 400 feet of saturated alluvium. So, what we're beginning to do is to be able to define the pathways from the repository and get a better handle on how much alluvium those pathways will encounter. And, we conclude at this point that, at least, some of the pathway to 20 km will be through alluvium. Now, I said 20 km. We'll get to the implications of the new regulation which shows a different compliance boundary at the end of this talk. Wells drilled to the north will allow us to reduce this uncertainty further.

Okay. Hydraulic head and water level data have also been collected and interpreted. I don't know if the packets have the diagram in them. They do; good. The diagram on the right in the packet shows the new potentiometric surface that's been drawn on the basis of the new data. The bottom line conclusion of that is that those contours are relatively unchanged with the incorporation of the new data. But, we have determined some interesting information beyond just a potentiometric map from these data and that is in Wells 2-D and 2-DB where we have measurements of the head and the volcanic rocks, 706 m, versus the head in the underlying carbonate aquifer of 715 m, an upward gradient. We've seen that in Well UE-25P-1, but within the model domain, that was our only observation of upward gradients. And, now, this data is supplementing that in suggesting that it's a more comprehensive feature of the groundwater system that the gradients are upward. The reason that's important is that that should confine the transport of radionuclides to the uppermost strata that are encountered. In other words, the plume should remain relatively shallow if you have upward gradient. And, it seems to be a more pervasive feature of the groundwater system or, at least, our understanding has improved that that's what it is.

We have a problem with the graphics here. I don't know if we have the wrong talk. But, what I'm going to do is since you have the packets, since you have all the right graphics, I'll ask you to focus on those as I give the talk. In the area of modeling, we have alternate conceptual model studies. There are various aspects of the groundwater system that basically have conceptual models associated with them. One of them is the large hydraulic gradient to the north of Yucca Mountain. In the previous modeling, we've had a low permeability feature which we've said is what gives rise to that large hydraulic gradient.

In these analyses, what we've done--actually, I've got viewgraphs here. Why don't I use them? I suggest we turn that one off and I'll give a talk that I'm used to giving which is with me in control of the viewgraphs. Sorry.

As an alternate conceptual model for the large hydraulic gradient, we've replaced a feature which I don't draw on this map with a more distributed zone of low-permeability rock in the north. The results of that analysis which are documented in the SSPA is that we generally get better calibrations to the head data in the low gradient region where the flowpaths from the repository would occur and we get somewhat more southerly flowpaths, as well.

That's shown on the next slide. This is comparison of particle pathlines predicted by the model from the original calibrated model and this one with the more distributed low-permeability in the north. They're both calibrated models. So, they're both representative of the data as we know it, but the travel pathways are somewhat more southerly in this new model. So, it's an area of uncertainty that we've looked at, found to be fairly minor, but has cleaned up some of our analyses in the sense of exploring different possibilities.

The next topic is sensitivity analysis of a cooler repository design. The black dots here are the original repository footprint; whereas the red dots are what was tested as a larger repository footprint to look at whether the travel times, the transport times from the repository, are the same or different, and also the pathlines. The conclusion from that which I'll show in the next slide is that there's really no significant impact of the footprint of the repository being somewhat larger.

That's shown here. Original repository pathlines, increased footprint, basically they overlay one another and the transport times in the form of a breakthrough curve really show no difference in the two.

SAGÜÉS: In the previous, which was which?

ROBINSON: Sorry. This is the original TSPA calculations. The larger repository footprint, you can see the red extending further south. That's the representation of the new footprint.

SAGÜÉS: Thank you.

ROBINSON: Sorry.

Moving to unquantified uncertainties, I divide these into rock properties, transport properties, and flow parameters. I show with an asterisk those which were used in the supplemental analysis, the TSPA. That's our supplemental analysis, the Volume II. There are others that are in a sense a work-in-progress. We're still looking at some of the distributions that we're studying, waiting a little longer for additional data, but the ones that we did include were bulk density and certain sorption coefficients that have been measured and the distributions of those parameters adjusted as a result of that. What I'll show is one analysis that looks at a different representation of the groundwater specific discharge.

Our method of doing that in a saturated zone is to look at multiple realizations and roll-up the information in the form of a median transport time. This is a histogram of median transport times within the saturated zone only for the original model and then the refined or the updated modeling. So, we're looking at multiple realizations and looking at those distributions of travel times to the 20 km compliance boundary. This is for neptunium in this case. The transport results show a narrower distribution of predicted times and the primary reason for that is that we're using a narrower range of values for the groundwater specific discharge, plus or minus a factor of 3 from the mean value as opposed to, in the previous analysis, plus or minus a factor of 10. There's a description in the document as to the rationale for that change in the distribution.

I'm going to move to multiple lines of evidence. I, like the Board and other people in the saturated zone, think that this is very important to confirm and to further justify the use of our models and that there is more than just running TSPA calculations. We need to have data that confirm that the models are on the right track. I'll show some examples in terms of interpretations of Yucca Mountain data itself. These are data that don't go into a formal calibration target for a model, but nonetheless, we need to make sure our model is reproducing. There's also in the document, although I won't have time to talk about today, an examination of independent analysis of the Yucca Mountain saturated zone. We're comparing our approaches really with those of EPRI and NRC and that's documented in the SSPA. Then, there's natural and anthropogenic analogs, as well. But, like I said, in the interests of time, I want to focus on the interpretations of Yucca Mountain data.

The next slide shows the hydrochemical and isotopic analyses. The assumption or the premise here is that there are trends in the chemical data that add an aerial extent that can be used to delineate the large-scale features of the groundwater flow patterns. If you see compartmentalized data with high chloride concentrations in this region and over here and lower ones following a path from north to south, then that tells you something about the flow patterns that's independent, if you will, of the other information. Now, we do this with things like chloride, but also other species, as well; conservative species usually or reasonably conservative species, as well as stable isotopes which tell us about basically the recharge fluids that are entering the saturated zone. We conclude from that and it's documented in the SSPA and also in an AMR that the flow model results that we get with our particle tracks are consistent with the flow patterns that you deduce from the hydrochemical data.

In addition to flow patterns, there are conceptual models for how radionuclides might travel through the system. In the past, you've seen talks about the C-wells experiment which looked at fractured volcanic tuffs and transport mechanisms. We now have single-well tracer tests in the ATC that we can look at to attempt to validate the porous continuum conceptual model for alluvium. That's what these results capture. Bottom line, we believe the results are consistent with this porous continuum conceptual model which, as I said before, is important in terms of performance. Basically, your pumping tracers into a well, chasing it with tracer free solution, and then allowing it to sit there for a prescribed period of time and then pumped back out at the same well.

We see in a fractional recovery during pumpback of different tracers that have different diffusive characteristics that the curves overlay. That's consistent with a porous continuum model without significant or large amounts of stagnant water that's interchanging with the fluid in the active flowpaths and the stagnant water. It appears to be a porous continuum and that's in contrast with the fracture volcanics where the C-wells data showed quite clearly that the opposite is the case. You have stagnant water and transport in fractures.

We also injected microspheres during these experiments. The recoveries are lower, and after flow interruptions, you get spikes of microspheres that are recovered in that fluid and that's consistent with the filtration models that we're using for colloid transport. So, the microspheres serve as an analog for colloid transport and the filtration model is consistent with that data.

The final example of multiple lines of evidence are the ^{234}U/^{238} ratios in the saturated zone fluids. It's probably better in your packet. But, here, are--got some water flow across here, but here are the data around Yucca Mountain. They show anomalously high ratios compared to the surroundings of ^{234}U/^{238}U ratios. And, that means that that's sort of an artificial tracer for the recharging fluid that's recharging at Yucca Mountain. It's distinct from what's around it. In terms of the flow model, the presence of those anomalous ratios support the notion that there's a hydrologic isolation and slow movement--not zero movement, but slow movement--of groundwater directly beneath the mountain; as well as the fact that as you go to points south and the ratios are lower, that's consistent with a dilution mechanism in which dispersion and mixing are taking these high ratios along the flow path and reducing them as you go downgradient. So, there is qualitative consistency with our notions for how the chemicals should migrate from Yucca Mountain. There are uncertainties associated with this and the other analyses and that's why you really can't formally calibrate using data such as these, but nonetheless, they're important.

Final topic is the 40 CFR Part 197 standard for Yucca Mountain. It is specified as being given latitude, but no greater than 20 km. So, the new boundary that we are going to start using is straight across at about 18 km from the potential repository and then following the 20 km arc on either side of it; as opposed to the 20 km boundary that we've been using so far. So, what we wanted to do was to supplement the SSPA analyses with some calculations to show what the impact of that might be.

Results of that are shown here for C-14, a conservative species, and Np-237. The newer compliance boundary results in somewhat shorter travel times due to the shorter flowpath length in the alluvium. What I'm plotting here is a cumulative probability distribution for 100 realizations of the median transport time comparing the 18 km fence, the new final regulation, with the 20 km. The effects are somewhat greater for neptunium because in addition to effective movement, you also have sorption in the alluvium for neptunium.

So, in summary, I've shown newly collected data that I believe, in general confirms the previous representation of the saturated zone, but it's given us a little bit more robust description of the saturated zone. New flow model representations in which we've examined conceptual model uncertainties is another feature of the work in the SSPA document. Larger repository footprint didn't have much impact. I showed the unquantified uncertainties analyses in which we in some cases will have narrower ranges of SZ behavior compared to previously. I focused on the multiple lines of evidence. Those provide independent confirmation of the various conceptual models and assumptions that go into the modeling. And, finally, the new EPA standard prescribes a slightly closer compliance boundary and that resulted in somewhat shorter transport times in the saturated zone.

Thank you.

SAGÜÉS: Thank you. Any questions from the Board?

CRAIG: Craig, Board. Yeah, this was interesting. I think the alternative different ways of looking at it gives you some feeling of comfort with respect to the uncertainties. You calculate different ways and get some more answers, that's encouraging. The overall message seems to me to be somewhat similar to the message of the last presentation on the UZ; namely, unless you have some kind of holdup mechanism, the transport times for the water themselves are below 1000 years. That was true for the UZ and it appears to be true for the SZ, continues to be true for the SZ, which means that the previous message remains unchanged that the primary holdup mechanisms for the whole repository are chemical type holdups and new metals, C-22 and titanium. Is that correct? Am I stating the SZ portion correctly?

ROBINSON: With respect to the SZ, I wouldn't put it as starkly as you do because I think that there is an uncertainty range that we're dealing with in which some of the predicted transport times--in other words, there's some probability, a finite probability, that transport times would be greater than what you just referred to.

CRAIG: That would be very interesting, but I didn't see that in your presentation. It's true that for the neptunium, you have ranges, but for simply mechanical transport of water, I didn't see ranges that seemed significant.

ROBINSON: If we look at C-14 which is as close an analogy to water transport as we can get here, this is a considerable range. This is a cumulative probability. So, 20 percent of the realizations are up in the thousands of years. To get the median value, it's true in this range right here, but there is a significant probability based on the uncertainties that we have that travel times would be longer than that.

CRAIG: Yeah, but 1000 years is up in your high probability range?

ROBINSON: Well, yeah. Yeah.

CRAIG: Yeah?

ROBINSON: Uh-huh.

CRAIG: That's why I used 1000 years for my number.

ROBINSON: Okay.

PARIZEK: Parizek, Board. Do you consider the new data from the regional groundwater flow model from the USGS as flux boundaries and so on? Does that mean in the current runs you're making or in the--

ROBINSON: Yes. Just to amplify on the question a little bit, there is a corresponding model at the regional scale that we are in close communication with and basically we use that model and the site-scale model to prescribe calibration targets for how much water is coming into and going out of our model. The reason for that is that our model doesn't have natural hydrologic boundaries. It's carved out of space in the form of a rectangle. So, we need some way to set those boundary conditions and that's what Dick is referring to. The version of that model that was used in these calculations is a previous version. We're getting updated hydrologic and geologic model results from that, but they are not in the work that I presented here.

PARIZEK: On Page 9, you show the footprint difference and you use the southern addition as an example. Would you get any advantage if you used Grants Ridge as an expansion area west of the fault zone rather than going south? It's not obvious what difference would that make.

ROBINSON: Yeah, we haven't explored that in the way of calculations. I believe the thing to keep in mind when doing that is that you can crank through a model run and see what your model says, but the first thing I would do is really take a close look at the data that support that model before really believing the model results too much. The model results that we present are based on data that we've collected for the purpose of the repository as it's currently envisioned. If we move outside of that, there are implications in terms of data collection that might be needed to really characterize the rock to the same degree that we've characterized it in the assumed region that we have right now.

PARIZEK: In Figure 7 with the revised direction of flow is slightly more southerly, as you show. Does that make any difference in terms of where the saturated alluvium might come into play compared to what you have in the original calibrated model or is that still--

ROBINSON: Not really because basically, although it's more southerly once you get down into here, the flowpaths, you know, if you were to overlay them, are pretty similar down in the region of where it transitions to alluvium.

SAGÜÉS: Okay, very good. We have a question from Board staff, Dr. Diodato?

DIODATO: Yeah, Diodato, staff. Thank you for the presentation. I had a couple questions. One, on your Slide 9, the breakthrough curves, looking at those travel times for, I guess, the conservative species, strike some people maybe as being extremely conservative. So, the question would be is there any field evidence to show breakthrough times, breakthrough curves arriving that quickly at that distance from Yucca Mountain?

ROBINSON: The purpose of these calculations was to compare two different systems; one with a larger footprint, one with a smaller. What I didn't want to do is wrap in a bunch of different processes, even those that might occur and affect these results. I didn't want to include them because

I really wanted to look at flowpath differences. So, these curves are curves with no sorption and no diffusion in the matrix of the volcanic tuffs. So, I didn't mention that. It's in the documentation. That's the primary reason for the shorter travel times that you see in this plot right here is that there was no matrix diffusion because I wanted to have a look at basically just the hydrologic processes in comparing these two.

DIODATO: I understand that you didn't include those things, but it shows maybe the sensitivity of the boundary conditions which might change now with the new USGS model.

Then, the next question was you mentioned the EPA regulation, 40 CFR 197, the new one. And, in that, they have a revised amount of water for dilution going from 1285 acre feet per year to now 3000 acre feet per year. I was looking at your Slide 7 which shows the original calibrated model paths and the new alternative one. Is there any model evidence? Like how much flux would be coming through exactly where these breakthrough curve--where the particle tracks occur? Have you looked at the actual flux through the predicted zone of contamination?

ROBINSON: You mean the specific discharge?

DIODATO: Yeah, for water.

ROBINSON: Yeah. It's--gosh, I don't remember exact numbers, but on the order of a meter per year.

DIODATO: So, what is that in acre feet? I'm not going to do the conversion. I don't know. It's a meter per year. So, we can do that conversion and see how that compares with 3000 acre feet.

ROBINSON: Yeah, that's right.

DIODATO: Do you think that that 3000 acre feet number, how will that compare in your mind with a meter per year?

ROBINSON: Basically, it is a quantity of water that essentially encompasses the entire plume as it's flowing through there.

DIODATO: So, you think this plume is a 3000 acre feet per year discharge at that point?

ROBINSON: Well, yeah. I mean, the regulation is consistent with the sort of calculations we're making here in the sense that 3000 acre feet per year is enough water that you're really mixing--you would be mixing the entire plume. If you really drew that much water out of the system, you would be getting a good fraction of the entire plume, yeah.

DIODATO: Or more.

ROBINSON: Or more which means that in that sense you're getting a little bit of dilution by drawing that much water. Now, that's the groundwater protection portion of the regulation. The individual protection standard remains a lower level and we use a distribution in the PA calculations for that. Distribution of acre feet per year.

SAGÜÉS: We'll have to continue that one later. We have one more question from Dr. Reiter, Board staff.

REITER: Leon Reiter, staff. I don't know if it's a question directed toward you, but you raised the issue of the large hydraulic gradient. At the last meeting of the Board, here was some questions about to what extent this is included in calculations and we learned that, indeed, that in the TSPA/SR Rev OO, the presence of the repository over the large hydraulic gradient was not included. They used the old EDA-2 design. In your present calculations of both the hot and the cold, do you include the presence of the large hydraulic gradient and how would this affect, for instance, travel times in the unsaturated zone for that particular part; two, the existence or nonexistence of drifts underneath the repository because in some situations it would be flooded when the water rose; and three, temperatures in the saturated zone, particularly after 600 years and you still have high thermal pulse, but your water was 100 meters higher, how would that affect the temperatures in the saturated zone and how would it reflect the movement of vapor upwards?

ROBINSON: Okay. There's a variety of questions there, some of which obviously touch on the unsaturated zone, as well as the saturated zone. I guess if you're talking about water rise, you know, there's--we can't distinguish between the two because it's a combined system. But, the analyses that have been done--and we intend to look from the unsaturated zone perspective at this in more detail through calculations that we haven't done yet--but basically, the analyses have shown that you still will be unsaturated, I believe, at the repository level even if the repository were extended somewhat north. It's a few hundred meters and you're starting to creep into the large hydraulic gradient area, but I think the analyses performed by other people--and Rob's going to help me here, I think--is that it will remain unsaturated.

HOWARD: A couple of key points for you, Dr. Reiter. I had a feeling this question was going to come up at this meeting as it has several times in the past few months. I'll see if I can address it a little bit in more detail here.

The issue with what's the effect of the large hydraulic gradient and the water table rise, in the science and engineering report, we suggest that the maximum water table rise is about 120 meters. For a future climate induced water table rise, the northernmost emplacement drifts of the repository layout near the large hydraulic gradient would remain about 80 to 90 meters above the water table. And, Bo gave you a little indication of that yesterday, as well. For the TSPA/SR, the effects of the water table rise were incorporated by including a uniform rise of approximately 120 meters below the repository. So, in the implementation, the rise in water tables included for all climate states that are within our current climate states. So, it persists after 600 years. The effects of the water table rise on a the large hydraulic gradient, as you have pointed out, are not explicitly included in the TSPA implementation.

Although as Bruce just explained and as Bo explained yesterday, in the process level work that we've done in Volume I, we're including the footprint and the large hydraulic gradient in those analyses. Performance implications--and Bo gave you some yesterday, Bruce gave you some today--it can be drawn not by looking at TSPA calculations because we want to look at our process understanding that, the TSPA implementation, first.

The percolation fluxes are generally lower in the northernmost regions of the repository. The number of radionuclide particles released in the repository that reach the water table on a median infiltration case is generally lower in the north.

What else can I say? Oh, the flexible design approach, you know, we were looking at the thermal implications mainly and the SSPA weren't looking at the explicit design features. We use these footprints as a point of departure. The approach for taking--if there's a performance implication, the flexible design will allow us to make adjustments and, in fact, if there are performance related implications of a water table rise in the northernmost drifts, we can incorporate that by changing the design footprint. So that's, in general, how we're addressing this issue. There is a discussion of it in Volume I.

With respect to the water movement from the thermal analysis, we have not done any recent evaluations with what those effects are. So, I hope that helps a little bit. If Bruce or Bo wants to adjust my thinking on that, I would certainly welcome it.

SAGÜÉS: Clearly, we are not going to have time for more discussion. We're going to go straight to the last presentation. The next presentation is by Dr. Bob Andrews from Bechtel SAIC and it's going be on integration of supplemental science analyses and models into the supplemental TSPA model.

ANDREWS: Thank you. What I'd like to do now is do a little transition from the talks that we've heard this morning and yesterday afternoon which, more or less, walked through each of the salient rows of the table that Bill started you with yesterday at 1:00 o'clock. We've walked through the system through the UZ, the engineered barrier system, the environments on the package, the package, inside the package, then back into UZ with UZ transport, unsaturated zone transport. We have not touched on disruptive events, the volcanism and seismicity issues. There are some aspects of those included in the SSPA and some changes that Peter talked about yesterday afternoon and we have not touched on the biosphere. There were some slight modifications in the biosphere. Generally, those differences were on the order of a few percent different from what was in the TSPA/SR Rev O.

What I would like to do is then kind of pull it together, those aspects that have been talked about and say, okay, now, many of the presenters have talked about performance implications of that part of the system within their part of the system and now we're going to talk about moving their parts of the system into the total system and the integration of all of that information, that new information. Some which didn't change what was in the previous models and some of them which did change what was in the previous total system performance assessment models. After lunch, Mike Wilson will walk through the results, your subsystem type results and system results. You saw an initial preview of that yesterday, but Mike will go into more detail on those.

So, I want to walk through the integration of this new information and why it was incorporated into the thermal system performance assessment and look at some examples of things that were not included. The things in this table, you know, these Xs and the Xs with Ts--Rob had Xs with Ts yesterday--this one just has the Xs, the things that move from the component level into the system level. We've talked to the Board and Bill Boyle presented to the Board back in January roughly 30 things that we were looking at. You know, the number of rows on this table actually grew to 80-ish, but not all of those. They all have a performance implication, but not all of them have been incorporated into the total system performance assessment. I think it's worthwhile spending a little time on what made it into the total system performance assessment and what did not and why.

Let's go onto the next slide. So, one of our goals in doing the TSPA part, there's a lot of goals that Bill and Steve talked about with respect to each individual scientific component, those goals being a more thorough investigation of unquantified uncertainties; you know, evaluation of additional closure processes, look at thermal operating modes, and the potential effects it may have on coupled processes and therefore on performance implications of those coupled processes, and finally other lines of evidence independent of total system performance assessment that helps support that case. The previous speakers went through synopses summaries of that information. But, now, we want to talk about the last part of it. What are the implications from a total system perspective of this new information, the thermal information, the high-temperature operating mode versus low-temperature operating mode, and the new scientific information, some of which being to address the unquantified uncertainties issue.

The third bullet on here is a very important one. The Board pointed out to us in, I believe, September, again in their formal summary of the issues in January, that coupled processes, some aspects of coupled processes were making it difficult to discern what could be potential differences for different thermal operating modes. That's my paraphrase, you know, you have the actual quote in your Board letters. And, it is true in the Rev O there were a number of thermal dependencies that were in the models and in the analyses, but there were other thermal dependencies that were not in the models and analyses. So, we added a number of thermal dependencies into the models analyses--many of those were talked about by the previous speakers--in order to see, when we combine all these thermal dependencies or as many as we thought were relevant--and we'll talk about which ones were relevant here in a second--into the analyses doesn't make any difference at the system kind of level.

Let's go onto the next slide. So, how do we evaluate that the first goal was to evaluate the potential significance of new information, whether that be unquantified uncertainty or that be new scientific information collected in the field or in the laboratory? It gave a lot of different ways within the supplemental science performance analysis. One is at the component level, at the individual process level, an analysis was done, comparisons made. Inclusion or exclusion of thermal dependencies, inclusion or exclusion of coupled processes didn't make any difference. They also used other lines of evidence to support their conclusions in Volume I. Most of that has been talked about here. There's other examples in the actual text. In addition to those and Mike is going to talk about some examples of these after the break, is a number of one-off type sensitivity analyses were done. You bring the new information, whether that the unquantified uncertainties or thermal dependency or thermal operating mode, into the system level and see just from that one thing did it make any difference or not and do it at the kind of subsystem level. This is not dramatically different from the barrier importance, barrier neutralization type analyses that are documented in the baseline TSPA which the Board has reviewed.

The next part of the approach is to combine all the relevant component models that did change into that TSPA and we're going to talk about that here in a second. And, finally, the reason for doing that, as a side note, was to make sure we had as many thermal dependencies in the models, in the analyses in order to discern whether there was any significant difference at the system level of different thermal operating modes. You have seen, you know, from Bob MacKinnon, from Bo, from Pat Brady thermal dependencies of their models and we want to incorporate those thermal dependencies into the TSPA, those that were different or caused a difference.

Let's go onto the next slide. Okay. The next set of viewgraphs relate just those things that were not included. Those things that, if you will, didn't make it from Volume I, even though they had a performance implication and the potential performance implications described in Volume I, that did not make it into the total system performance assessment. One, there are some examples where, yeah, a revised model was done in order to address unquantified uncertainties where the probability of that revised model was deemed to be too low or sufficiently low that its inclusion in TSPA was not warranted.

Secondly, and a lot of examples of this exist, at the component level, that is at that subsystem or component level, the change was insignificant. Yeah, there was a change. There is a difference, but that was deemed to be insignificant and therefore not propagated into the system level analysis. There were some examples that even if they were brought into the system level, they were deemed insignificant. So, they did not make it into the totally revised model.

There are other examples. In fact, we were just talking about one a little while ago on the drift shadow effect, very real, very physical, but still somewhat uncertain. Some of the uncertainties that Dr. Bullen mentioned led one to say, yeah, that could be real. Perhaps, we should include, you know, ultimately in future TSPAs, but right now the level of uncertainty was such that incorporating it would be on the non-conservative side, even perhaps not on the optimistic side, but on the non-conservative side; i.e. leading to lower doses.

There are other examples of those. You know, transport from the package to the invert, Bob MacKinnon talked very briefly about that. Very promising, but not--and I think some results were presented to the Board in May on the potential effect of that. Those results are in the document, but it was not incorporated into the supplemental TSPA model. In addition, some of the things that Jim talked about with respect to the UZ transport model, the change in the UZ transport model were not included in the supplemental model.

And, a final reason--and Greg had an example of this is the model--in this particular case the generalized corrosion model that Greg talked about and Jerry Gordon is going to talk more about this afternoon--is still at the conceptual stage. It's not ready for incorporation into the final TSPA.

I believe the next slide is corrupted. So, we're going to switch to a few examples. In fact, you have this in your handout. Now, this is an example, in fact, that Bo presented yesterday and I believe all my examples either Bo or Bob MacKinnon talked to yesterday. The reason I chose these examples--there's many other examples that individuals have presented; you know, Pat Brady had the pH that didn't change much, etcetera. The reason for putting these examples is I wanted to focus on the coupled process ones. The coupled processes and interaction of processes in the rock, interaction of processes in the drift is very important and correctly and appropriately accommodating that coupling between thermo-hydrologic. Thermo-hydrologic-mechanical and thermal-hydrologic-chemical processes may yield different performance results. Some of those were not incorporated in the supplemental TSPA. In other words, they were screened out, if you will, at the process level. Their significance deemed to be insignificant at the process level.

So, I'm going to walk through four of those. Two of them are thermo-hydrologic-chemical ones and two of them are thermo-hydrologic-mechanical ones. There are two different scales. At the mountain sort of scale, a number of analyses were done that are described in Chapter 3 of the supplemental science performance assessment and these are also changes at the drift-scale and those are documented in Chapters 4 and 5.

The first one is a mountain scale THC where the analyses that have been conducted--generally, you have to look at the top plot there or it's discernable in the actual handout and in the text--indicate that the fracture porosity has changed by less than 1 percent. There's a change of less than 1 percent. The fracture porosities are on the order of roughly 1 percent in the current models. So, a change of 1 percent is insignificant given that it's 1 percent. So, if you will, it's a .01 percent effect. That's insignificant with respect to permeabilities and insignificant with respect to transport characteristics of the fracture-matrix interactions and therefore wasn't included. These particular results are at the higher thermal operating mode which would be more stressful, you know, of the system for the change in porosity due to thermo-hydro-chemical effects. It is true and the document points out that those effects are expected to be less at the lower operating modes. So, cooler temperatures would have less of an effect on thermo-hydrologic-chemical.

I believe, John, the next slide works, right? Yeah, okay. This is another example that Bo showed part of the story of. He showed this figure with respect to stresses. These are thermo-hydro-mechanical now. Stress changes in the mountain induced by a high-temperature operating mode and a low-temperature operating mode. This is the effect now, you know, of that stress change. Stress itself doesn't do much, but stress does change or could change rock mass properties and fracture permeabilities. And, in fact, it does. You know, for the high thermal operating mode, these analyses show that the vertical permeability which would like of affect distribution of flow vertically through the mountain change by roughly--or maximum change by roughly a factor of 10 in the upper 100 meters and decrease by less than that same factor of 10, you know, slightly above the repository. So, you can go divide by 10 or multiply by 10. But, these changes, the overall rock mass permeability, the fracture permeability of the rock mass, is quite variable and quite uncertain. So, these changes are well-within the bounds of the variability and uncertainty of the ambient, if you will, rock mass that we have. So, they were also not incorporated. The changes are expected to be lower at the lower thermal operating modes. The less stress you're applying, the less stress would be less change. But, the fact that even at the high thermal operating mode, they didn't change significantly and these are all new analyses in the supplemental science performance analysis document and implied no need for incorporation into the TSPA.

The next slide goes through thermo-hydro-mechanical changes at the scale of the drift. The left hand side is for thermo-hydrologic processes alone and the right hand side for thermo-hydro-mechanical processes. The bottom two slides show the changes in flux around--or not change, but the absolute values of the flux around the drift incorporating THM processes or not incorporating THM processes. They are virtually identical. The blues are slightly different. These are at a time of--it's not shown on here. I believe, it's the time of about 1000 years. I'd have to verify that. But, again, the changes in the flux induced by the thermo-hydro-mechanical response of the rock mass is relatively insignificant at in this case the high thermal operating mode. So, therefore, the need for inclusion of them to evaluate the differences between high and low in the supplemental TSPA are minimal, and therefore, they were not included. In other words, it was a process evaluated at the process level, we believe, appropriately and at the process level differences discernable, but no need to include those in the supplemental TSPA.

Let's go to the next example which is also a TH which is again a THC effect at the drift-scale. I believe these are results that were in Bo's presentation yesterday. We looked at two different conceptual models because there is conceptual model uncertainty in many of these aspects and these happen to be at the high thermal operating mode. But, again, the changes in permeability are roughly factors of 2, you know, in and around the drift and changes in permeability of roughly factors of 2 around the drift are insignificant given that the range of permeabilities used in the seepage calculations which is where drift-scale kind of changes become important, the range of permeabilities used in those analyses is about 4 as a magnitude. So, factors of 2, given that I have 4 as the magnitude variability, in the fracture permeability in and around the drift imply no need for inclusion of that change in the TSPA. So, therefore, they were not included in the TSPA.

Let's go onto the next slide. Oh, there are a number of other examples and they're probably too numerous to mention. I probably shouldn't even have attempted to try to put them on a slide. But, the other examples, you know, are in the table. You know, if something has an X, if you will, in the first three columns, there's some new information or a new analysis or an unquantified uncertainty, sensitivity study conducted, or a thermal dependency that was added or might have already been there and it may or may not have propagated into the last two columns of the table.

Let's go onto the next slide. Some of the key things that were included and this is also a partial listing. The table has the complete listing and Peter's presentation yesterday had the complete listing of all the things that were incorporated into the total system performance assessment in order to evaluate the total system implications, as opposed to the component level sensitivity analyses. So, this is a subset, if you will, of that. These are the key ones that changed. And, when Mike shows you the results, the reason for the change in the results and the sensitivity and the insights that are gained from that change in results, Mike will talk to you about. But, generally, as it's come out fairly clearly in the presentations, a number of things changed in the waste package. There were things changed in the in-package transport and in EBS transport that do have a significant change on the results. The solubility also was mentioned as a significant change and that was fairly clear how much that would change from Pat Brady's talk.

Let's go onto the next slide. The other reason, of course, for trying to make sure we included things into the supplement TSPA because one of the goals, as I said, in the beginning of the supplemental TSPA was try to do as meaningful a comparison of high versus low thermal operating mode as we could. That is the Board's Question #3, if you will. So, things that were not thermally dependent in the TSPA/SR Rev 0, we wanted to make thermally dependent. So, we developed models of that thermal dependence. The first one was a number that has been discussed several times back last year of how independent seepage was from the thermal condition. So, we made seepage dependent on the thermal conditions. A lot of the analyses done of thermally-driven seepage at both the high operating mode and the low operating mode. Bob MacKinnon talked about the next one, the change in evaporation and liquid saturation in the drift itself being driven by the thermal operating mode. So, not just the humidity, but also the evaporative fluxes and the liquid saturations were included in the supplemental TSPA.

Bob talked a little bit about the in-drift chemistry and its change as a function of thermal operating mode and temperature. That was not included in the Rev O TSPA. And, finally, one that was mentioned this morning by Greg was the thermal dependency--I probably shouldn't say TH effects there, I should say just T effects on corrosion rates--the thermal dependency of the corrosion rates. In order to discern does the thermal dependency or potential thermal dependency of corrosion rate, how does it change the results and would you discern any difference from a high-temperature operating mode from a low-temperature operating mode? There are a number of other models. Rob in his handout, you know, all the ones with the T in there or those models or analyses that were thermally dependent incorporated in the supplemental TSPA.

Let's go onto the next slide. Of course, all the things that are directly thermally dependent, you know, the humidity, the temperature on the drip shield on the package, the temperature inside the package, the fluxes, the invert saturations, and the seepage fractions, those are all direct thermally dependent. So, those were, of course, incorporated. Bob MacKinnon showed you the range of results, you know, from those that were directly incorporated into the supplemental TSPA.

Let's go onto the next slide. So, we've incorporated those things that we deemed necessary sufficient to incorporate into the supplemental TSPA. The things that had a basis at the component level for not inclusion in the supplemental TSPA were not included. You know, our prioritization was driven by thermally dependent issues because the main reason for developing that model was to look at the differences between high and low thermal operating modes and the effects of unquantified uncertainties. So, the Board's first and third issues, if you will. And, some of those things were either not thermally dependent or were insignificantly thermally dependent. So, they were not included.

So, I think I'll stop there. Mike will present the results after lunch and entertain any questions, Mr. Chairman.

SAGÜÉS: Dr. Andrews, once again, you have proven to be the dream presenter. You have brought us back to schedule.

The first question is from Dr. Christensen.

CHRISTENSEN: Clearly, there was one or more significant changes in terms of the effects of thermal coupling. It was illustrated actually in the presentation Peter Swift gave yesterday, his Slide #13 which simply shows the dose response with the effects of the early weld failures and the thermal coupling to corrosion. I wanted to ask this question earlier. There's a dramatic shift in the timing of the response in that curve. And, my question really pertains to the difference between the high and low thermal modes in that there seems to be some significant departure early-on, the first 10,000 years. The next roughly order of magnitude of time between 10 and 100, they roughly are the same and then about between 100 and 1,000,000 years, the thermal operating modes differ again; in some cases, almost an order of magnitude as you're beginning to see these climatic events having an effect. I guess, my question is why does thermal operating mode matter after 100,000 years? What's happening that is making those curves differ in that time period? Why is it showing up? It seems like so distant from the early temperature effects. Is it something that happened early-on that now we're seeing it later or--

ANDREWS: I'd have to defer to Mike.

BULLEN: Bullen, Board. This is actually to Mike's

Slide 11 that he's talking about.

CHRISTENSEN: It's actually Slide 13 or about--

ANDREWS: It might be better to wait until Mike stopped and--because Mike's going to go through the result set.

CHRISTENSEN: If it's coming up later, that's fine.

ANDREWS: Yeah, why don't we pass it if that's okay.

CHRISTENSEN: Okay.

ANDREWS: I just gave him a good warning. That's nice.

CRAIG: There's clearly been a lot of progress. Craig, Board. There's clearly been a lot of progress in responding to the Board's concern and I certainly have to congratulate you and the program on what you've done in sharpening up the conversation. I still am feeling a certain sense of frustration in terms of actually getting my brain around what the uncertainties are. Now, there are a lot of additional runs and one can compare how much the runs differ, but there are issues relating to what are the--what are key parameters that really matter and what are the uncertainties that are associated with those? I raised such an issue early-on where you took a uniform distribution with a very sharp cutoff on the ends. That was simply stated with numbers, but no uncertainty, no discussion as to where that came from. Or to take the one that was talked about earlier today that I felt was really interesting was in Joon Lee's Figure 5 which talks about the stress corrosion cracking. Here, you have some data points which show no effect, but there's huge ambiguity about how you extrapolate from 2500 hours out to 10,000 or 100,000 years and how you do that matters. If you're trying to fit a functional form, you really would like to have some way of figuring out what kind of uncertainty bounds to put on the parameters. That kind of a conversation, it seems to me, needs to take place. This is a good lead-in to the conversation of what we've been doing today, but it doesn't get to the job in my judgment.

ANDREWS: I think to be fair, we have to look at many of these changes, many of these rows, and I think the one you're mentioned from Joon is a good example. Those things that changed in the stress corrosion cracking representation is in some ways alternative representations. You know, you compare A and B. We didn't sample, if you will, between A and B nor did we go outside B. You know, I think Joon and Jerry talked about that plus or minus 80 percent as the one example as opposed to--not plus or minus 80 percent, but 80 percent of yield than going from 80 to 90 percent versus the very conservative assumption that was made in Rev 0 and acknowledged in Rev 0 of between 20 and 30 percent. Jerry pointed out there are data that indicate it could be even higher, significantly higher, than the 80 to 90 percent, even up to 200 percent, I believe. We could have sampled, if you will, from 80 to 200 and seen, well, what's the difference there? You might better look at it as Model A versus Model B and a comparison between those two models where the thing that's different now is the percent of yield strength before you initiate a crack. One is 20 percent, 20 to 30 percent, and one is 80 to 90 percent. We could have said new data indicate it's up to 200 percent, lower bound is 80 percent. So, sample between 80 and 200 percent. But, I think you can get at the same question by just looking at the two, if you will, almost point values, 20 to 30 versus 80 to 90 as an alternative representation, alternative model. And, many of the examples throughout here, yeah, from a total system performance assessment perspective, they can be kind of parameterized. But, at the individual process level, it's really almost alternative models. You know drift shadow effect is an alternative model, an alternative representation. You know, the fracture-matrix coupling that Jim talked about is really an alternative model, an alternative representation. Those, you'll be comparing A versus B, not the full suite of sampled distributions.

CRAIG: That's a good answer. I wouldn't look at the drift shadow as being an alternative model. I think that's not the way to conceptualize it. I think it came out very clearly in Jim's presentation that actually it was in the model, but you didn't see it because of the grid size. And, now, they went back and changed the grid size and you can see it. So, it seems to me that that's an example of an effect which is a very, very real effect and it may not be developed enough for you to include, but the reality of it seems highly likely and that has a huge advantage that you can test it experimentally.

ANDREWS: Uh-huh, that's true.

SAGÜÉS: Okay. Dr. Bullen?

BULLEN: Bullen, Board. Actually, I'd like to echo my colleague, Paul Craig's, comments and compliment the project on sort of your quick response to the four issues we've raised and maybe to the PA program, in particular, because you've done sort of a Herculean effort. Yesterday, I may have sounded like I was complaining that we have 1300 pages to review, but I wanted to basically state that, you know, we're very pleased that you're as responsive as you are and that we get these multiple volumes. Never maybe as timely as we'd like, but we get these multiple volumes of new information.

Along those lines, I actually wanted to ask a couple of questions that are more philosophical. Do you think the results are more representative of actual performance based on the fact that you've used the incorporated unquantified uncertainties and you've adopted maybe more realistic models? So, do you think you're getting closer to what the predicted or real performance might be?

ANDREWS: Well, I don't know what the real performance is. I'm not so omniscient. But, I think there's a lot of good work to address, you know, the degree of conservatism that we acknowledge we have in the Rev 0 TSPA. And, in a couple of cases that don't come out here, some optimism. You know, one or two cases have potential optimisms. I'm not saying we've evaluated every single conservatism because we haven't, but at least I think we have a better understanding. We, I hope, have provided greater insights by these exploratory analyses, insight producing analyses, of what could be the range of possible performance. I don't want to say it's better or worse than what was in TSPA/SR Rev 0. I look at them as all exploratory, you know, trying to explain, collaborate degree of conservatism or look at that potential optimisms that may have been in the Rev 0 TSPA document which also uses the basis for the science and engineering report.

BULLEN: Bullen, Board. Just a quick followup, maybe even a yes/no question. Do you think the effort was beneficial to the program?

ANDREWS: Yes.

BULLEN: Thank you because we get criticized when we ask these questions and you just said it was beneficial. So, maybe it's not as bad as--

ANDREWS: Well, I shouldn't have maybe just stopped at yes. I think, you know, everybody acknowledges there's uncertainties in the analyses, in the models, in the TSPA. A large number of those uncertainties require additional information, additional analyses, additional data, additional testing. We've talked about some of the additional testing done and analyses done in the last three or four months not only to address the Board's concerns, but to address our own desires to more fully evaluate the uncertainty. The testing is not over. I mean, there are a number of tests the Board is well-aware of that are usually briefed every three months from the testing organization, from Mark Peters and others, on the status of the ongoing testing to help confirm the coupled processes, to look at the corrosion processes in greater detail, the waste form testing continues, and I think Russ Dyer presented to the full Board in January about the kind of continually learning and stressing and pushing the models. I think that's what we've done in the last three or four months.

SAGÜÉS: Thank you. Jeff Wong?

WONG: Another easy question. On Slide 5 on your fourth bullet, you say that the model is sufficient uncertain that the inclusion would be non-conservative. Can you kind of explain how you arrived at sufficiently uncertain? Was it you were afraid that the potential upper bound was too optimistic? Sufficiently uncertain, it varies four orders of magnitude, five, two times, four times? How did you make that decision?

ANDREWS: It was more qualitative and subjective. I think a lot of good work--you know, I'll take the example of the drift shadow effect. This was mentioned to the Board back, I don't know, eight months ago or so as a potential, very conservative assumption that was in the baseline TSPA at that time. A lot of work has gone on, you know, to look at it from an analysis point of view. What would be the potential, if you will, benefits for the inclusion of that particular process into the TSPA. A lot of work went on at the component level, but as was pointed out, there's a lot of good empirical observations, a lot of good conceptual understanding of it, but no direct field, if you will, observations at Yucca Mountain about the degree to which it is or the degree present or its extent. You know, how far does it extend? Is it one meter, 10 meters, 40 meters beneath the drift itself? How much effect is there of lateral processes or heterogeneities, both fracture heterogeneities and matrix heterogeneities? So, we kind of believed, although it's still very, very promising and ongoing work is being planned and conducted from an analysis point of view and from a testing point of view, we believe at this point in time--I mean, all these are kind of snapshots in time--at this point in time, inclusion of it, direct inclusion of it, would be on the optimistic side. So, it was more of a--I don't want to put a number on it, a factor 2, a factor 10, but we believe inclusion of it was sufficiently uncertain at this time. That it was better to exclude it. Part of it is a flat out of time issue, too. You know, we had to stop someplace and, as somebody said yesterday, we kind of stopped in the April/May time frame on new information and incorporation of that directly just wasn't possible in the time.

WONG: So, there should be a sixth bullet down there, ran out of time.

ANDREWS: Yeah. The model is still conceptual is kind of the not enough time bullet. Yeah, you're right.

WONG: Okay. Because I was going to ask you the model is still conceptual was simply the model didn't have any data behind the idea.

ANDREWS: There is analytical data, but not directly measured data to support it, yeah.

WONG: Okay, thank you.

SAGÜÉS: We have the last question. Dr. Di Bella, Board staff?

DI BELLA: Bob, you said that you incorporated temperature's effect on corrosion rates into the TSPA for SSPA and my question is aimed at exploring to what extent you made that incorporation. Greg Gdowski in his talk this morning, I thought rather persuasively, showed how deliquescence could occur at temperatures from, I guess, 120 to 165 or maybe even higher than that because of the possible presence of magnesium chloride or calcium chloride salts. Then, I take it to mean that corrosion could occur, generalized corrosion, in that temperature interval from 120 to 165 and that--of course, the switch was turned off for TSPA/SR. And, I didn't really hear anybody say that indeed the temperature dependency of corrosion rate was implemented in this study being reported today for that temperature range, 120 to 165. Could somebody clarify that it indeed was or it wasn't?

ANDREWS: I believe it was included at temperatures higher than 100 degrees C, but I should look to Joon or Greg to confirm that for me.

LEE: Actually, we did additional sensitivity runs having the (inaudible) at 150 degrees C (inaudible) magnesium chloride and that analysis was not and this is being added to the Section 7 as we speak. And, the result is not much different. The reason is that the time period we have the high-temperature is much shorter than (inaudible) waste package (inaudible) and that the (inaudible) will be added into the Section 7.

DI BELLA: So, what we've seen today are what you had up this morning?

LEE: Yeah.

SAGÜÉS: Okay. We are testing time to go ahead and turn the meeting over to Dr. Bullen. I would like to thank very much today's speakers. We are going to have now, as announced earlier this morning, a 15 minute period of public comment, Dr. Bullen?

BULLEN: That's correct.

SAGÜÉS: And then, there's also a change in the scheduled time which we reconvene in the afternoon. Is that --

BULLEN: Yes. And, in fact, I would like to have a short public comment period. We have a couple members of the public that cannot stay until later this afternoon. I want to reiterate that there will be an additional public comment period at the end of the meeting. This public comment period will run until 12:15 at the latest. We will reconvene at 1:15. So, there will be a one hour period.

I have two people who are signed up, Mr. Grant Hudlow. Grant, did you want to speak now or do you want to speak at a later time?

HUDLOW: I'd like to now.

BULLEN: Now, okay. So, you're up first. So, if you want to come to the microphone here or come to the podium, it's your choice.

HUDLOW: I'm Grant Hudlow and I'd, first of all, like to thank you for coming here so that we can watch you struggle to try to save the nuclear industry. In view of the previous messes that the DOE has made, I'd like to ask you to think how much or imagine, if you will, what kind of pain you would be in as you're reacting to listening to inexperienced amateurs struggling to design the system in your field. That's what's happening to me as I look at the canister design, for example. In my field, people contain 300,000 barrels a day of flowing material with far more dangerous components and conditions. This is rather amusing to watch people make the mistakes that you've made. Does the Board feel that experienced people in this design are unavailable or too much work to find them? I'd like to hear what you have to say about that or does the Board need additional expertise itself to even realize that people with industrial experience are needed in this project? For example, we have the 360 degrees C inside the canister and now that's gone. That's not even being considered. The difference between 100 or 200 degrees C and 360 degrees C with the material that's in there is an amazing difference. What happens when your cooling system fails and you hit the 360 degrees C if you don't have a design for it? The reason I mention that, it's very simple to design for that and it isn't going to cost any more than what you're already seen.

On another subject, I'd like to see an analysis of why the plutonium migrated a half of mile west in 50 years on the test site and why the radionuclides migrated out of the tuff similar to Yucca Mountain from Los Alamos to Cochety (phonetic) Lake in a matter of months. We're saying here that it's going to be thousands of years to do that and we have two examples, one in case of years and the other one in the case of months where it did the same thing.

Thank you.

BULLEN: Mr. Hudlow, thank you very much for your comments. Actually, you very aptly pointed out that we aren't all omniscient and know everything on the Board. When the Board does need extra expertise, we are in the habit of hiring consultants and experts to supplement what we've done. Not necessarily at this meeting, but at subsequent meetings and previous meetings, we have done that. So, in answer to your question, the Board does try and address those issues realizing that we have our own shortcomings.

Our second public comment is from Ms. Sally Devlin. Ms. Devlin?

DEVLIN: Thank you, Dr. Bullen, and again welcome to the Board and everybody that I see that I've seen for eight years, it's always fun having you here in Nevada. I wouldn't be at a meeting and I miss Russ Dyer because after every meeting, we told dirty jokes and shaggy dog stories. So, I brought you one. That is a hobo and his dog, dirty people, were walking down the street by a bar and the hobo says, "Come on, Shep, let's go on in and maybe I can get a free drink." So, he says to the bartender, "I've got a talking dog and the dog's name is Shep, and if he can answer three questions, will you give us a drink?" And, the bartender says, "Yeah, I've seen this before. Go ahead?" So, he says, "Well, Shep, what's outside of a tree?" And, the dog says, "Bark." And, he says, "Very good. Okay. And, what does sandpaper feel like?" And, Shep says, "Ruff." And, he says, "Okay." And, who is the world's greatest baseball hitter?" And, Shep says, "Woof." And, the bartender looks at him and grabs them both and throws them out the door right back into the gutter. And, the hobo looks at Shep and he says, "Gosh, maybe it was Joe Dimaggio.

Thank you, thank you, old friends, good friends. That's very kind. But, I'm telling you this joke--everybody finally got it. Thank you, everyone. I appreciate that. Anybody else? Anyway, the reason I use this joke at this time is, as I say, we have to get back to Pahrump and then we're coming in for the peer review tomorrow, I hope, at Texas Station. But, the way I feel is everything that you said and everything you said for years, it might be Joe Dimaggio or it might be Ruth and everything that you have presented, it's plus 3 or minus 3 or it's this or that. And, every one of your things is a disclaimer of one sort or another. It's like the ads you see on tv. Don't take this drug if you're pregnant, if you're nursing, if you're falling down, or you're dying because it might have an effect on you. And, as the public has tried to understand all this stuff and I really do understand an awful lot of it, I'm saying to myself if I just came and I heard--you know, you put in Archie's comics and now you've put in Josephine and all kinds of new fun. And, I really do enjoy it and I say, gee, they didn't talk, at all, about my bugs. Now, what if my bugs come in a canister with the rods from Hanford and they're falling apart containers and they go up to Yucca Mountain? How are my bugs going to mix with the other mountain bugs?

And so, if you don't look into more microbic invasion--I haven't heard a word about it in these days and I hope I hear a lot more about it because I love it that the Josephine and the Alloy 22 which are nickel and my bugs, as you know, love nickel. So, this is just one of the very basic thing that means something to me that you again are disclaiming because it's there.

The other thing is with the disclaimers and that is you talked about the SSPA. We haven't seen that. I did read the huge engineering report which I carried yesterday and I'm saying to myself they're lovely diagrams, they're lovely pictures, and I just love it that you give us choices of four of this or four of that. Now, you want to put in four swimming pools. Where are you going to put the water? What are you going to do when it leaks? What if you have a compression fault? And, we do have them all the time. Remember, we're basin and range and the water is always sinking. I was just out at the test site and the water sank from a well. Get the documentation on Well-54 in Area 5. It just happened over in China. So, we're seeing things and I'm saying to myself I hope you continue modeling for another 20 years because I think you're anything, but ready, to go to law with this and I'm very interested to see what comes tomorrow.

The last thing I want to leave you with because, as I say, this whole thing to me has been a disclaimer and I want to thank Linda and Linda for bringing the Congressional Report because Dr. Cohon, who I just love, he wrote four things that he wants this to contain or be a what-have-you. The fourth one is development of multiple lines of evidence to support the safety case of the proposed repository.

Well, safety means a great deal to me since I live in the shadow of Yucca Mountain, as far as I am concerned, with no transportation and you don't talk money which again I don't care for. But, he says the lines of evidence being derived independently of performance assessment, and thus, not being subject to the limitations of performance assessment. And, this again is DOE-ese. If you can tell me what that little paragraph said, I will eat my hat. Can anybody tell me what that meant? This whole thing is full of these kinds of disclaimers that are not in English. So, I do hope that by the time we have the next conference--and I'm sorry that Abe and other people aren't here to hear this because they're the ones doing this and I just met another attorney who is writing the licensing. It cannot be ironclad when the whole thing is disclaimed. So, may I hope that more and more, rather than go 10,000 years out, my concern is the disclaimer for the first 300 years and what you'd love to do to us, not only with all of the different--the 97 maybe metric tons, the different containers, and the different stuff. I have asked Alan Benson any number of times for the information of what the DOD stuff is and I will say it to my dying day, you cannot put classified waste in my mouth because if ever there was a disclaimer that is absolute unacceptable to the public, that is it.

And, with that, I'm only going to leave you with one other thought which I'm taking right from our contents of the meeting today. That is this unqualified uncertainty, being the great toastmaster that I am, is unacceptable. I want something that is not unqualified and that certainly is not uncertain. And, when you can insure the public that you have both of these, we might be content and not hear yelling at you like I always do.

Thank you again.

BULLEN: Thank you, Ms. Devlin, and in fact, I don't think you ever yell at us. You're always forthright and very honest.

The comment I'd like to respond to is maybe we are a little bit DOE-like or DOE-ese when we ask our questions. If you take a look at the fourth point that's in our letter to Lake Barrett and in our most recent report to Congress, I think we're asking for things that aren't tied directly to computer modeling. So, we would like things like natural analogs and we would like other lines of evidence or other explanations of physical phenomenon that don't necessarily get changed when you tweak a certain parameter in a code that will allow you to match data to a model. So, we're looking for a fundamental understanding. We appreciate completely the fact that this has to be open and transparent to not only us, but to the public.

The other one that I'd like to address is we've been using the term "unquantified uncertainties" and not "unqualified". Unquantified means--

DEVLIN: I know--

BULLEN: Yeah. No, no. But, unquantified in our case means we would really like to see some numbers ascribed to it. Okay? So, that's just sort of a little bit of a help. I hope it helped you out in understanding what the Board means. And, if you ever have a question, just give me a call and we can discuss it and that wouldn't be a problem

Right now, I would like to maintain the one hour lunch hour. So, regardless of the fact that I said we'd be here at 1:15, please, come back at 1:10 at which point my esteemed colleague, Dr. Christensen, will reconvene the meeting.

(Whereupon, a luncheon recess was taken.)

AFTERNOON SESSION

CHRISTENSEN: Our chairman tells me we have a quorum of Board members. So, we'll go ahead and begin. I think that others will join us momentarily. I'm Norm Christensen. I'm a member of the Nuclear Waste Technical Review Board and I'd like to welcome you to this final session of the Board's Performance Assessment and Repository Panel Meeting devoted to the Department of Energy's supplemental science performance analysis or SSPA report.

We'll start off this afternoon's session with a presentation by Michael Wilson of Sandia National Labs on Total System Performance Analyses and Results based on revisions to the individual input models that we discussed yesterday and this morning.

After that, we'll have a series of presentations on how the SSPA provides insights on the Board's four priority areas mentioned yesterday. Kevin Coppersmith of Copper Consulting will address the quantification of unquantified uncertainties and conservatisms in performance assessment. Jerry Gordon of Bechtel SAIC will address progress in understanding fundamental processes of corrosion. Jim Blink of Lawrence Livermore National Labs will address the comparison of high and low-temperature repository designs. And, Ardyth Simmons of Lawrence Berkeley National Laboratories will address the development of multiple lines of evidence. Bill Boyle will cap off the afternoon with concluding remarks on the SSPA.

After that, we will have a session of public comments, and if you would like to make comments, I'd encourage you to please sign in the back of the room at the table near the back door.

Mike, I invite you to the podium. As I mentioned, Mike Wilson is Sandia National Lab where he's been for the past 12 years. He's currently a principal member of the technical staff working on total system performance assessment for Bechtel SAIC.

WILSON: I didn't make a list of people contributing to this, but make no mistake, I'm only representing a lot of other people here.

Let's go onto the next slide. I'm going to talk about a little reminder of what we mean by the TSPA base-case model and then some of the one-off sensitivity analyses that we did with the base-case model. Then, a little reminder of what the TSPA supplemental model is and then close out with a number of results for it.

Next slide. Okay. What I mean when I say base-case model is the Rev 0 model that came out last December. In that model or in that report, we considered two scenarios, the nominal and the igneous disruption. Nominal basically means everything that is pretty much expected to happen, and in this case, it means everything except igneous disruption. I'm not going to be discussing igneous results, at all, in this talk. Peter touched on them yesterday and most of the people who are really interested in it are at a different meeting on that subject today. So, I'm not going to get into it, at all. The technical basis for the base-case model and results are in the process model reports and AMRs that came out last year, over 100 AMRs and, I don't know, what is it, 10 PMRs, something like that. As you all know, in some of the areas, uncertainty was addressed by me as conservative assumptions or simplified models. That's been a point of contention and also the design that was considered the base-case for that was relatively hot and includes temperatures above boiling for hundreds of years.

Next slide. Sensitivity analyses, this is some of the one-off sensitivity analyses that have already been mentioned by several people. I'm going to just give four examples. The first three are the ones that I think when we get to the supplemental model results and, in fact, that you already saw yesterday, the first three are the things that have the greatest visual impact on the changes going to the supplemental model. To start with, there's what we call the extended climate submodel. That is in the base-case model, the climate was only changing for the first couple of thousand years, and after 2000 years, there were no climate changes in the model. And, since then, a more realistic long-term model has been developed including cycling between glacial and interglacial climates over 1,000,000 years. This is the result. Because of the glacial climates, every so often you get these spikes in the releases and in the doses associated with the glacial climate. They don't show up as well, but they're little downward spikes associated with the interglacial climates when it's dryer. The very first glacial climate is represented by this increase here and the first interglacial is by that decrease. So, this is showing that going from the base-case model to including the more realistic climate model, you get basically more spiky. And, if you recall back to what Peter showed yesterday, the supplemental model results are spiky like that.

Next slide. Okay. Now, in the course of the work for the SSPA, there have been a number of important things looked at connected with the waste packages and I wanted to pick out one to show and the one that has the biggest effect is this issue of the temperature dependence of the general corrosion model. In the TSPA base-case model, there was no temperature dependence of the corrosion rates. In the modified model that is used for this analysis here, as was mentioned earlier this morning, the corrosion rates match at about 60 degrees C and at temperatures higher than 60 degrees, the corrosion rates are faster, and at lower temperatures, the corrosion rates are lower. And, the crossover is around 10,000 years. That's when the temperatures go below 60 degrees C. In fact, it's a little before 10,000 years. So, you have faster corrosion for the first 10,000 years, but then for all the hundreds of thousands of years after that, the corrosion rates are slower and the net result is that you push the waste package failures out to later times and you push the doses out to later times. And, it's a fairly dramatic effect. There's a couple hundred thousand year delay in the bulk of the doses and it also reduces the highest value that's attained.

Next slide. Okay. The other thing that has the big visual impact in looking at the graphs that Peter showed yesterday are the change in the model for early waste package failures. In the base-case model, there were no failures before 10,000 years. There was an analysis of the potential for early failures and, you know, it was based on a literature study of different kinds of reliability studies and the conclusion of that was that they didn't think there would be any thru-going failures, that there would be defects that would go partway through the welds, but then it would still take them thousands of years to corrode the rest of the way through and actually fail a waste package. More recently, some further analyses has been done that was discussed this morning having to do with possible effects of improper heat treatment in the final closure of the waste package. In the course of that analysis, they suggested a higher probability for early failures. So, with this higher early failure probability, you end up with releases starting before 10,000 years.

Next slide. Okay. Beyond those three which, as I say, I think are the ones that have the greatest visual impact and going from the old model to the new model, there's a lot of other things that were considered and has been said. Some of them have made significant changes, some not so significant. I picked out one of the other relatively significant ones to show and that is the inclusion of sorption in the engineered barrier system that is within the waste packages and in the invert under the waste packages. In the base-case model, the transport within the engineered barrier system was modeled without any sorption. Putting in an estimate of the sorption makes this kind of a difference and it's pretty significant. There's something like a 20,000 or so year delay for the first breakthrough of the doses and then also is lowering the dose a little bit. I think by the time you get to the peak, it doesn't really lower the peak, but you get this lag. The most important effect there is the reduction of the concentration within the waste package and the source because of the sorption onto the corrosion products inside the waste package. The lower concentration there gives you lower diffusion gradients and lower advective terms and everything. And, that's more important than the lag you get in the transport out of the waste package and through the invert.

Next slide. Okay. Now, I'd like to go on to the what we are calling the supplemental model which is a roll-up of a lot of these different things. Bob Andrews went through a list of a lot of them and we have heard about them in the last day. A lot of different things have been investigated over the last several months and a number of them have been put into this rolled-up model to give a better understanding of how they interact with each other. In those one-off studies, you can see the effect in isolation, but in order to see the effect on the while system, you need to see the interactions between the different ones. And, the technical basis for this is in the SSPA report in Volume I. These results that I'm going to be showing are documented in Volume II of the SSPA. There are results for nominal and igneous, but once again in this talk, I'm not going to discuss the igneous. Many of the modifications of the model were done to make the subcomponents more realistic. In other cases, it was to better quantify the range of uncertainty. We look at a range of thermal possibilities embodied in these two thermal operating modes that you've already heard a lot about; the high-temperature operating mode which is essentially the same as the base-case design and then the low-temperature operating mode which is intended to keep the waste packages well-below boiling.

Next slide. Okay. And, this is the kind of basic comparison of doses that Peter Swift already showed yesterday. As I said, I think the three big differences between the old base-case model and the new ones are, number one, it's much spikier; number two, for the main rise in doses, you get this big lag of several hundred thousand years, but there's also a low-level of doses going back to early times. You can see that there is not a big amount of difference between the two thermal operating modes. There's some differences, but they're much more similar to each other than they are to the old base-case result. I think it's dangerous to read too much into the small difference between the high and low cases because, remember, Peter showed the horsetail plot showing all the realizations and there's a range of several orders of magnitude in the doses here. When you average over that, you end up with this small difference. And so, as I said, it's dangerous to read a lot into that small difference, but there are things in the model for which that makes sense. Number one, the preclosure period is longer in the low-temperature case. It's 300 years instead of 50 years. And, we model post-closure performance so there are no failures of waste packages during the preclosure period and that gives you an extra 250 years of delay there. But, probably more importantly is there's some thermal dependencies in these early waste package failures. In particular, there's a relative humidity threshold for when the corrosion is modeled to begin and it's at that point when you get these early failures occurring and that gives you some extra lag because the relative humidity is lower in that low-temperature operating mode. The differences out here at the late times will show later, more clearer when I show the waste package failure comparison. This is because of a difference in the waste package failure results for the two cases and I'll talk more about the reasons for that when I get to that.

Next slide. Okay. The next three slides, I show what radionuclides are contributing to the dose for the different cases just to give you a feel for what matters. At the early times, it's carbon iodine and technetium, the very fast, very mobile, and very high solubility radionuclides that breakthrough the fastest and give the dose at the early times. At 10,000 years, essentially, the dose is all from those very fast ones. As has been said before, in the base-case, there was no dose at 10,000 years because the waste packages hadn't failed yet.

Next slide. You get a lot more radionuclides coming through within 100,000 years. The first big difference you notice is that there's a lot more technetium in these new results than in the old results. I think that is more just that the amount of neptunium is smaller and that the technetium is higher. The neptunium solubility has gone down somewhat. So, the relative proportion of technetium is higher. Another important difference is that in the base-case model there was much more ^{239}Pu coming through at 100,000 years. It's a very small amount in the new result. And, that is primarily because the newer (inaudible) work has lowered the (inaudible) for plutonium. So, plutonium is much less important than it was. Another thing you can see of note is that there's quite a bit of ^{231}Pa in the high-temperature case, but it's very little in the low-temperature case and that is just a fluke of a particular time slice. This is near the time when the ^{231}Pa is starting to break through and you catch the breakthrough in this one, but not in this one. If it were a little later, as in the next slide, the Pa is very similar in them. The big difference you see here is in the base-case model. There was a much greater of ^{230}Th. ^{230}Th is so low in these new results that it's not on here, at all. There's a little bit of ^{229}Th and that comes from a change in the biosphere calculations. The biosphere does conversion factor. In the base-case, it was too hard for ^{230}Th. Just because of the way things evolved, the whole extension of the results to 1,000,000 was done a little bit late in the game and ^{230}Th was added at that time and they didn't do as thorough an analysis of it as they did for the other ones.

Next slide. Okay. Now, I'm going to go down into the different components of the model to show some of what's going on that influences those dose results. I haven't emphasized it up to now, but I will now. For every one of these plots including ones I've shown before, there's a wide range of results. If you look at all the realizations, you get these things that we call horsetail plots, but I'm only showing the average curves to illustrate the differences between the models, but you should keep in mind that there's a big range of results within each one. This shows the climate and infiltration part. You get in the base-case model just a flat curve out here, but in the supplementary model, you get these big infiltration spikes during the glacial climates and then you get these big downward spikes during the interglacials.

Next slide. This shows the flow rate of seepage getting into the drifts for the three models. And, there's two parts that are important. Delay part is just driven by that climate and infiltration part that we just saw. So, it has the same spikes in it and it's basically the same for the two operating modes because all the temperatures and everything are the same by the time you get out to such late times. At early times, there are differences in the way the thermal effects are being included between the base case and between the supplementary model and there's differences in the two operating modes because their temperatures are different. In the base-case model, we were not reducing seepage because of the vaporization zone around the drifts. In order to just make life easier and because it was conservative, we just didn't do that reduction. And so, in the base-case model, there's actually a pulse of seepage into the drifts during the hot period because above the drift there's a pulse of water and we just let it continue on into the drift. Now, in the newer models, we are reducing the amount getting into the drift because of that dryout and also there's some subtraction from these curves because of evaporation inside the drift at the drip shield. Bob MacKinnon mentioned that there was a model for that now yesterday. You see that the high-temperature case has lower seepage at early times and the low-temperature case doesn't go as low. There's a slow increase over time here and that's because of that evaporation term decreasing over time. At late times in the new model, the kind of base seepage rate is lower, but you do get these spikes that go up to higher values.

Next slide. Now, this is another part of that and this is showing the fraction of waste packages where there is seepage. The previous slide was showing the flow rate for the locations where there is seepage. And, you can see that the number of places where the seepage has gone up and going from base-case model to the supplementary model, but at those places where there is seepage, the flow rate is lower. And, there are several things that go into that. Number one, these represent the fraction of places that has seepage any time within the simulation and that includes those glacial climates. This one, didn't have those glacial climates. So, that's a part of it, but it turns out that that's not the most important part. The most important part are really some changes in the seepage model that were put in having to do with the flow focusing factor going down, as Bo talked about yesterday, and some other assumptions having to do with episodicity of flow. Those are the things that are making this number go up and they also contribute to making those flow rates go down.

Next slide. This shows the distribution of drip shield failure, the first failure of the drip shield. And, there's essentially no difference between the high and low operating modes. The failures are later than they are in the base case and that is almost entirely just because of the change in the way the uncertainty and spatial variability are handled. Joon Lee mentioned this morning about the partitioning between uncertainty and variability and that is the thing that pushed this average curve out to later. If you see the individual curves, you would see that the range of things is fairly similar between all three of them, but on average, they're a little later.

Next slide. Okay. Now, we come to waste package failure curve which is one of the most important drivers of the system. And has already been brought up, the failures of the waste packages--of most of the waste packages are much later in the new model, a couple hundred thousand years later, although you do have this small fraction of early failures. Of particular interest is that the failures are a little bit later in the low-temperature operating mode than they are in the high-temperature operating mode. That is because of that corrosion temperature dependence that was put into the model. It seems surprising to see it showing up out here because the temperatures are essentially identical between them by the time you get there, but it's all driven by what went on back during the hot period. Don't take these numbers to heart, but the idea is that the waste packages corrode through like three-fourths of the way during the hot period, but then getting to that final fourth of the way takes a very long time because the corrosion rates have gone down. In the high-temperature mode, the corrode a little farther through than they did in the low-temperature.

Next slide. This shows the amount of water flow getting into the waste package in one of the groups. This is a combination of the curves I showed earlier for the seepage into a drift and the waste package failure because the amount getting into the waste package in our model depends on how many drip shields and waste packages have failed and also how they have failed. That is, there's a dependence on the amount of the area that is open and available for flow-through. And so, it's pushed to later times because the waste packages are failing later and it's lower both because of the lower seep flow rates and because of the waste package failure being later and fewer of the waste packages have failed by that time.

Next slide. Temperatures, that's a straightforward comparison. The low-temperature mode, obviously, is much lower temperature than the high-temperature mode. The difference between base-case and high-temperature operating mode is mainly because of the update in the thermal conductivity of the lithophysal units. The conductivity went down and also the temperature goes up a little.

Next slide. This shows the relative humidity at the waste package for the three cases. This first part is during the preclosure period and it's not realistic because the thermo-hydrology model doesn't include the removal of moisture by the ventilation system. It includes removal of heat, but not removal of moisture. That doesn't really matter because those preclosure temperatures and relative humidities aren't used for anything for modeling post-closure performance. The other thing that is of interest, I think, is that the low-temperature mode not only has lower temperature, but also lower relative humidity. Bob MacKinnon talked about that yesterday afternoon.

Next slide. Then, to close, I want to show a comparison of the releases from different parts of the system to get an idea of how the radionuclides move through the system. There are curves for the release rate from the waste form, then from the waste package, then out of the engineered barrier system, and then from the unsaturated zone to the saturated zone, and then 20 km out in the saturated zone. And, we could spend all day discussing all these little wiggles and bumps and peaks and valleys, but I merely wanted to get across the comparison between the different curves. The story--this is for technetium. For the base-case, we had a very straightforward story. The curves for waste package, EBS, UZ, and SZ essentially were overlaid showing that there was very little residence time in the parts of the system outside the waste package. There was some delay in the waste package mainly because at early times, you had these very small openings, only crack openings in the waste package which took a long time to diffuse through. You have a much more complicated picture in the new results. You can see that qualitatively the low and high-temperature cases are pretty similar. What I want to point out is that you can see the effect of the different barriers more because there's better time resolution since they're failing early. You get the waste form release here and then the waste package release comes in a little later and the EBS release comes in a little later, and then the UZ release and the SZ release, each a little later indicating for the initial breakthrough, you have on the order of hundreds of years for each of those. But, this is like the very leading edge of the breakthrough curve for each one. To see what the more average behavior is like you need to look out at this part and out here you see there's still a fairly important lag between the waste form release and the waste package release indicating that even out at hundreds of thousands of years, there's a pretty long residence time of technetium inside the waste package. But then, you don't see a significant residence time in the rest of the system. On a curve like this, a log plot, we can't really see what that residence time is. You know, if there's residence time of even tens of thousands of years, you just wouldn't be able to see it on this. Something that I should note is these three high spikes there that probably everyone is wondering about and those have to do with individual realizations where there are seismic events that fail the cladding very quickly and gives a spike of releases.

Next slide. Okay. This is the same thing for neptunium which has lower solubility and more sorption. So, it takes longer to get through the system. You have qualitatively a pretty similar story, but actually out here at the late times, you can see the separation more. You can see a significant lag between the waste package curve and the EBS curve. There's still not much separation out here between the EBS and the UZ and the SZ curves indicating that on the scale that you can see which is several tens of thousands of years, there is not a lot of residence. But, you can see that it is taking that kind of time to get out of the waste package and then out of the EBS. So, a lot of those things that have been put in the model having to do with additional sorption and things that reduce the diffusive pathways are causing longer residence in the engineered barriers.

Next slide. This is the same thing now for ^{239}Pu. Of course, this big decrease at late times is because it has a relatively short half-life. So, it's decaying away. But, even in the base-case model, you can see some spacing between the different curves indicating it has a much greater residence time in the different parts of the system and you can see that even more in the new model. Although with all these spikes being caused by the climate and by cladding events and all this, it's complicated enough that it's kind of hard to see it. But, you can see that, in particular, between the EBS curve and the UZ curve, there's a pretty big gap. It's turning into a vertical gap here because of that decay. If it takes hundreds of thousands of years to go through the UZ, it decays a lot during that time and so you actually get less ultimately getting out.

Next slide. Okay. To sum up, the most important driver for the changes have to do with the waste package. You have a small fraction of early failures of the waste package that gives you some dose at early times. The great bulk of the waste package failures occur later in making the --pushing the larger doses out later in time. There's only minor differences between the high-temperature and low-temperature operating modes in terms of doses. In some of the subsystem results, you can see important differences at least for thousands of years. But, by the time it gets all the way to dose at 20 km away, you don't see much.

And, conclusions having to do with uncertainty and uncertainty quantification will be in the next talk. So, I'll quit there.

CHRISTENSEN: Thank you. Board members?

BULLEN: Bullen, Board. Actually, you're right on one of the topics that I want to talk about because you mention that there's only minor differences between the high-temperature and low-temperature operating mode. Could you go back to 11 which is sort of the question that Norm Christensen asked this morning. Maybe it's just the artifact of something that you noted already. When you plot it in a log mode and you get out past 100,000 years, you know, a couple of thousand year time steps don't show up very well. But, if you look at sort of between maybe 150,000 years to like 300,000 years, that range right there, you're almost an order of magnitude lower in dose. Now, granted, you're not anywhere near the regulatory limit, but you're still an order of magnitude lower in dose for 150,000 years. Yet, just because of the way it's presented, you think that's a small difference?

WILSON: Well, it depends on what you mean by small. I think of it more as a lag. There's an additional lag because of the later waste package failure and--

BULLEN: Actually, I understand your lag and I do think that that--if it were me and you asked me if we wanted to wait 150,000 more years before it went up an order of magnitude, I'd guess yes. Okay? So, it's just one of those things that I found interesting and maybe you've downplayed a little too much that there's a little difference.

WILSON: Can I say something?

BULLEN: Oh, go right ahead. Yeah.

WILSON: I forgot to say earlier, I meant to point out, by the way, that the igneous doses in this early period are up about here, by the way. So, at the early times, these probability weighted igneous doses are much higher than these earlier releases from the nominal.

BULLEN: I just had one more quick question and then I'll defer to the rest of the Board members. Can we go to Figure 23, please? I'm happy that you described to me the fact that the relative humidity at short times isn't real and I believe that. I guess, I'd like you to explain to me again why at the low-temperature operating mode, the relative humidity is less. I mean, I--

WILSON: This part here?

BULLEN: Yeah. What's the physical phenomenon behind it?

WILSON: Okay. Well, I can make a stab at it. You know, Bob tried yesterday, but I'll take a stab at it, and if that doesn't work, then we'll get someone else to try it. My understanding is it has to do with basically with the radiative connection between the waste package and the drift wall. At these late times, the drift wall is essentially 100 percent relative humidity. And so, the relative humidity on the waste package is going to depend the difference in temperature between the drift wall and the waste package. At high temperatures, the radiative connection is more efficient. So, you get a smaller difference in temperature. So, you get a smaller difference in relative humidity. So, you end up with this counter-intuitive thing with a higher relative humidity at the higher temperature.

BULLEN: You're right. Is it counter-intuitive to me because as I look at something that's warm versus something that's cold, it's the warm thing that would appear to be dryer in my book. But, I guess, it--

WILSON: That's normally the case and--

BULLEN: But, I've heated up the wetter portion that made the relative humidity go up. So, it's the effective heat transfer as opposed to one being warmer and dryer and the other being cooler and wetter. So, it's effectively the heat transfer characteristics, not the temperature, absolute temperature.

WILSON: I think so, yeah. Right.

BULLEN: Okay, thank you.

PARIZEK: Parizek, Board. Page 24, the next page after that one, there are the spikes that you explain were earthquake related?

WILSON: Right. In the model, there's two basic modes of failure, the cladding of the spent fuel and one is--well, three, I guess. There's the early failures that are essentially already there at emplacement and then there's a possibility of failure from localized corrosion over time and then there's also a threshold for seismic events. In the model if the seismic event is above this threshold, then it's assumed to fail all the cladding. And, when that happens, you get this huge spike of the waste form. You know, all the cladding has opened up. So, in the accounting of the model, then there's all these radionuclides that are available for release. So, it's considered a waste form release. But then, it still takes a long time to get out of the waste package and everything. So, you don't see those same spikes in the downstream components.

PARIZEK: But, the spikes come after--the first ones you show are somewhat late. Why wouldn't they come earlier?

WILSON: Well, they're spread out in time. The seismic events are sampled and they can occur anywhere within the million year period. These are just three that happen to be particularly bad for whatever reason.

PARIZEK: And, another question with Figure 11 in terms of the juvenile failures. Is there any reason to believe there wouldn't be maybe more juvenile failures than those assumed to give us those results? I mean, that's still encouraging results, but suppose it's worse than that? How do you get rid of the concern that maybe there are twice as many or four times as many? Manufacturing gets sloppy or whatever.

WILSON: Well, I'm not exactly sure what you're asking. I guess one part of confidence in that is these doses are very low. So, you can still actually fail quite a few more before you start getting to a significant dose. If you're asking about how to build the waste packages to make that less certain, then someone else will have to talk to that.

CHRISTENSEN: Other questions from the Board?

SAGÜÉS: Since we have the figure up there, I think that maybe this is a pertinent question. It's a little bit broad, but here it goes. You know, as time progresses, the chances that the assumptions that were made to predict the different phenomenon are going to hold over dose rate for periods of time. And, is there anything in TSPA that is being done to--and, maybe, this will get into unquantified uncertainties question, but I might as well ask you now. That we do that, we put something in the predictions that would introduce a factor that gets bigger and bigger as time progresses, and then like this business, all the waste packages over there, we're making--assumptions are being made now of having big consequences, say, 100,000 years into the future, but of course, the chances that the corrosion rates are not going to be uniform, of course, all kinds of things later are going to happen. Is there anything that is being done or that could be done to introduce that numerically into these predictions so that the light would be getting broader and fuzzier as time progresses?

WILSON: Right. Well, I think that could be done. You could have uncertainty estimates of certain components that increased explicitly over time. As far as I know, we don't have anything like that in the model right now. Another example would be the climate and infiltration part, you might imagine that you could increase that uncertainty as you went out in time. We do not have anything like that in the model right now. You know, and corrosion is clearly an important one you could do something like that with. The problem, of course, is having the information to quantify this.

CRAIG: Paul Craig. Yeah, this is an interesting one and it does ask one to try and figure out what's going on. Let's see, we were told earlier that you're assuming one and occasionally two or three juvenile failures. It's a probabilistic distribution. So, if we multiply the one by 10^4, then we're up to maybe 10^{-1}, 10^{-2}mrem per year. And, we know from earlier presentations where the engineered systems were removed that you get doses up in the hundreds mrem per year. So, what's going on? I suppose what's going on is that the drip shield is there and the drip shield is assumed to be perfect forever or at least--

WILSON: No, not forever, but it is indeed effective during this period.

CRAIG: But, on the time span out to 10, 20, 30,000 years, the drip shield is considered to be perfect and that, of course, gets us back into the standard set of questions as to whether we really want to believe the probability distribution numbers for the titanium. This isn't the time to talk about that, but I simply want to make the point that it seems fairly clear as to why these numbers are so low. It's driven almost completely by an assumption that the drip shields work perfectly. Is that correct?

WILSON: Yes, that's why they're as low as they are because it's all diffusive release. There's no advective release. If there was a certain amount of advective release, it would push it up. It depends on what your assumptions are about and how much.

CHRISTENSEN: Dr. Reiter?

REITER: Yeah, my two questions, one is a follow-on to Paul. Can you go back to Slide 7, please? And again here what was looked at is just the one-off study assuming the same early waste package failures. And here, the releases are like 3 orders of magnitude higher. Now, what does this due to? The drip shield had the same sort of assumptions in both studies. You only had diffusive releases. This is due to the neptunium solubility or what causes the difference here; why so much lower?

WILSON: I wish Dave Sevougian was here, but--oh, he is here. I'll give you my part and then I'll let Dave give you a more authoritative answer. There are a number of things that have been put in the supplemental model to make diffusive releases more realistic and reduced diffusive releases. Those early releases are all diffusive and it's been decreased quite a bit in the supplemental model. Is there anything important besides that to say, Dave?

SEVOUGIAN: I guess sorption is a big factor.

WILSON: Oh, that's right. The sorption of the waste package is very important, as you saw in that other one-off for the early time, especially.

REITER: Technetium is probably the big--so it's really not solubility.

WILSON: Good point. Is there any sorption in the EBS for the technetium?

SEVOUGIAN: Sevougian. Yes, there is.

WILSON: Okay. So, it does affect the technetium.

REITER: And, just another quick question. I know you didn't show it. What's the effect of incorporating the way you did the drift shadow effect and we'd be dividing it up into advective and diffusive releases? How much--

WILSON: Well, we have one of these one-off analyses that shows that that's in the document, but as you say, I didn't show it. It actually gives a fairly similar lag to that one that I showed for sorption in the EBS. There's something like a 20,000 year lag for the initial breakthrough.

CHRISTENSEN: I'd like to follow with the last question. This is a question that comes from the audience, but it relates to one that I've wondered about. So, I'll use the audience question and then just augment it with a comment of my own. The question, you said that the outer Alloy 22 weld is assumed to be failed due to improper weld heat treatment. How do you model the inner Alloy 22 weld which is not being treated and accordingly isn't failed by this non-mechanistic event? Now, the followup question to that is isn't it unrealistically overconservative to assume both Alloy 22 welds are failed due to improper heat treatment? My additional question on this really has to do with what new information caused us to change our thinking about juvenile failures, what they are? How are we coming up with that? I'm just curious.

WILSON: Okay. Well, I really need a waste package person to answer this. I cannot answer it.

LEE: Okay. Joon Lee, BSC. As I discussed earlier today, in TSPA/SR we screened that potential mechanism based on low probability. We used the (inaudible) for criteria because the waste packaging failure is a one time event, not a recurring process. So, we use any priority less than 10^{-4} which is, you know, less than one out of 10,000 waste packages. That is what was done in the base-case. In this SSPA, we look at it again, the mechanisms, you know, again and then we did some detailed analyses for each event through a process. Then, we found that this improper heat treatment has a much higher consequence in terms of performance and release from waste packages so that we included that improper heat treatment into new update to the SSPA model. That's the story behind that. I'm not sure if I answered your question.

CHRISTENSEN: And, the question regarding two welds--

LEE: That case is--also, it has a bullet there. Even though outer failure has two--outer or inner lid, only outer lid will be (inaudible) by induction on any heat treatment. We know this by laser peening. But, since we couldn't quantify the effect of improper heat treatment of outer lid on inner lid, we just assumed both failed simultaneously if that affected the waste package. That is a conservative assumption we made.

CHRISTENSEN: Great. Thank you very much. Thank you, Mike. We'll move now to the next speaker who is Kevin Coppersmith. And, Kevin will be talking about the evaluation of unquantified uncertainties. Kevin is the president of Coppersmith Consulting and has long experience examining probabilistic hazard on certain characterization.

COPPERSMITH: I'm president, I'm secretary, I'm human resources manager, all of the above. Just one comment I wanted to make. There was some discussion--I've had a couple of people ask isn't this an incredibly busy time? It must be kind of a burden to have to worry about a TRB meeting right in the middle of all these deliverables, technical changes going on all around us, and so on. Many people in the project, believe it or not, take this actually as a challenge to be able to come to these meetings and make presentations. In fact, to some, the acronym NWTRB has become new ways to rattle Bullen. So, that's basically my goal and my challenge as we go through this.

BULLEN: Bullen, Board. Thank you, Kevin, I'm looking forward to it.

COPPERSMITH: First slide, please? The objectives of this talk we're giving here, I'll start first with reviewing the purpose. Remember, this study of unquantified uncertainties began some time ago. I think the concept of an SSPA was the twinkle in the eye of someone and so this was a study that was conceptualized some time ago and reported, I think, last fall by Abe Van Luik, reported multiplely by Bill Boyle in January and in May and so on. So, I want to review the purpose, approaches, what we're trying to do in the course of this study. I'll also talk about what the SSPA has in it relevant to the evaluation of unquantified uncertainties. Then, I'm going to summarize some of the system level conclusions that we can glean from what we've seen, so far, from the nominal system level performance. One of the problems, of course, of looking at everything in a rolled up form is unfortunate that we don't get an opportunity to see some of the subsystem level changes. For example, how does the change in a particular model or a conceptualization or alternative conceptual model or uncertainty distributions affect things like the subsystem level like, say, seepage flow rate and so on. Those results are included for the interested reader and I assume we have several here in Section 3 of Volume II and there's many discussions in there at the subsystem level. Part of the problem, of course, is rolling things up into the system level. There are some things that are more important at a system level and, therefore, we don't have an opportunity to see the implications of some of the now quantified uncertainties and so on. We'll look at conservatism. Conservatism here is basically--I'll just define and discuss more later--is the difference between our mean estimate of dose and the Rev 0 TSPA and our estimate of dose with the SSPA. And, I'll talk about why we use a mean. Usually, risk is a evaluated at a mean level. There are what we've called local conservatisms for a particular process model. Someone might have had a bound to their data and that would be a conservative bound for that particular set of data or to that particular investigator. Overall, right now, we're looking at things at a system level. How did they change from before and, of course, we've seen a lot of comparisons. But, that's the type of first order conservatism we might look at. Ultimately, I think it's important and I'll talk about where we go from here to look at it on a subsystem level, maybe more of a process model level at conservatisms and more realistic estimates.

Next. The purpose of the overall unquantified uncertainty activity, UU as it was affectionately known, is to evaluate the significance of uncertainties that weren't quantified in Rev 0, to develop insights into things like conservatism or non-conservatism, was is the significance ultimately? We know we have uncertainties in various input parameters. Which of those uncertainties drive the answer? What's the contribution of a particular uncertainty and input to the uncertainty and the output? Those are the types of considerations. And, of course, that's a common type of analyses to be done for risk analyses to look at what are the--not only the contributions, let's say the central tendency of risk, but what are the contributions to the uncertainty in that in that evaluation of risk? Again, we're doing this within the context of the TSPA, looking here first at system level results, ultimately subsystem. And, as I'll talk about at the end, we'll need to deal with some of those more gnarly issues. Those things that aren't quantified continue to not be quantified. How do we evaluate those? How robust will our future estimates be? And, finally, to develop guidance for future treatment. In the future, we anticipate that if we go into a licensing mode that all estimates, all evaluations of regulatory compliance, for example, will need a very careful assessment and evaluation of uncertainty.

Next. Some of the activities that have gone on over the last several months are shown here. The first is the identification of important unquantified uncertainties. TSPA/SR Rev 0 has a lot of uncertainty quantification in it. As you saw, for example, this morning discussions of the saturated zone, they're one group that, for example, has had probability distributions in almost all the input parameters. A lot of the activities that they went through was basically re-looking at those distributions. Bill Boyle in past talks in January presented a table that showed some of the important unquantified uncertainties. These are some of the areas that through evaluation and judgment were thought to be potentially important and were unquantified at the present point in time.

So, we started with that list and began to meet with technical principal investigators to review the current models was their basis. Members of the staff, I know Dave Diodato and Carl Di Bella and Leon Reiter and so on, at least, listened in on some of those discussions with the PIs. They went through where are we right now with the current models, where is their basis, are they realistic, and so on. It's important to remember that in the development and construction of Rev 0 that a lot of the guidance that was given on uncertainty treatment said the rules were essentially to do your best to quantify uncertainties. In the face of larger uncertainty, very few data--usually, those two are correlated--it is appropriate to bound or to give a conservative representation of that input, or for those conceptual models, many cases, conservative conceptual models have been used. We've had a lot of discussion of that.

A lot of the discussion of the process model presentations dealt with this, is what we did in Rev 0, it was a conservative estimate, perhaps not realistic, but conservative, nevertheless. What we're asking here is to change the rules a little bit, try to be more representative, more realistic. Along with the realism, of course, comes the need to quantify uncertainty. So, we went through a process then of, number one, making it okay to become more realistic. In fact, desirable and more towards the expectation, but also the need to characterize uncertainties. And, of course, for some people, uncertainty characterization is a tool of the trade in their evaluations and the types of work they do, uncertainties are routinely quantified. And, others, some of those who I think are probably farther from the geological sciences, we're used to hardly having any data than much of the geological science is. Therefore, judgment comes into play in the quantification of uncertainty almost on a daily basis. Others are much more data-driven and would use statistical approaches through uncertainty characterization. If I don't have much data, I'm at a loss for how to quantify uncertainty.

So, we had discussions about how judgment can be used to quantify uncertainty. Certainly, the parameter uncertainty, but also there are components of what we would call conceptual model uncertainty that are very important. To me, a conceptual model is a description of how a physical process actually works. Fracture-matrix interaction was given as an example and there are many others. All of our models have some sort of conceptual underpinning. In some cases, it's strong; in other cases, not. But, that discussion of conceptual model uncertainty is part of this, not just probability distributions and data.

The first steps dealt with developing more physical or representative models. There are many examples of that which we had discussions of better seepage models with thermal effects in them, episodicity to the seepage models, the evolution of in-drift chemistry, issues related to more recent data to help develop estimates of density of the alluvium that will be important, absorption, and so on. There are dozens of places that when you read through the SSPA where the models have been made more physically representative. This is the first step in this process.

The next is to quantify the uncertainty and I would say that we can't pretend to say that, in fact, all the new models now have new uncertainty distributions in them. That just hasn't been done. We've gone a long way, but I don't--I would say now the results received for the new SSPA do not contain a complete description of uncertainty. I think they've done the best they can do for this time frame, but I think it's going to continue on. Some of the quantified uncertainties that are many, often take advantage of new data. For example, some of the niche studies and other things that Bo talked about allowed for a better representation and the understanding of the lower lith to feed into the seepage model. We talked before about the use of drip tests and batch tests for neptunium and plutonium solubility models. These new data provide a basis and helped a lot with the quantification of uncertainty. And, it will continue on as we go through this process. The additional information can be fed, I think, hopefully and continue to be fed in a more comfortable fashion into uncertainty expressions as we move forward. The problem, of course, for those who've done work in risk analysis is the bounds are a very dangerous thing sometimes. If they're very extreme, the change of being exceeded may be extremely low. But, bounds sometimes are things that we say it just can't happen, it can't be any higher than this, and a single piece of data can violate that. So, there's some difficulties in that process. If we're describing a quantified uncertainty, new information can actually help give us a better description. We can welcome all types.

Finally, TSPA calculation and sensitivity analyses will show some of the system level results. There will be more one-off type comparisons that are found in Section 3 of Volume II and there will be some additional analyses that will go on this summer.

Next. Let me just give one example. A bunch of examples, you've already heard, but I thought I would throw in at least one here that sort of deals with where were we in TSPA Rev 0 and what came through in the reassessment of uncertainties? And, I think the whole issue of how the whole water diversion to EBS is a case where models were made, went from a conservative type model to one that is more represented. It doesn't mean that all of the conservatisms have been weeded out yet, but it means that they're headed towards a path of being more conservative. Let me just give you some examples of that. The first has to do with evaporation of seepage. This is water that has come into the drift, the potential for it to be evaporated such that it would not be water available to actually enter the waste package. It will be used to move radionuclides. In TSPA/SR Rev 0, that reduction in the amount of water that's available for contacting the drip shield's waste package is essentially ignored conservatively, clearly.

In the new evaluation that's in SSPA, there's a consideration of evaporation, mass balance type equations are used, a fraction of the heat is used to evaporate seepage. There are two distributions that are used for the higher and the lower temperature operating modes. So, this is a phenomenon that would have a thermal dependence as you'd expect it to. And, it partially reduces the amount of transport through the engineered barrier system. A more representative model with associated uncertainties is tied to it.

Next. Condensation of the drip shield is essentially no model for the process in Rev 0. But, in SSPA, there is a model that looks at whether or not, in fact, the drip shield was cooler or hotter than the invert. If it's cooler than the invert, there's a fraction of water that's evaporated and assumed to condense on the underside of the drip shield that you drip onto the waste packages. Right now, that fraction is assumed to just vary. It's just a sensitivity analysis to look at it, but it's a step towards developing, perhaps, a more realistic representation of this process.

Next. In terms of the geometric constraints on flow, how does the water get through a drip shield and ultimately get from on the waste package surface in to breach and into the waste form? Right now, there's conservative assumptions that say all seepage falls on the crown of the drift. There's a potential for a fraction of that water to get into the waste package. It's the function of the dimensions of the patches and so on. Basically, the model here is one that's very conservative. It allows for water to flow uphill from the sides of the waste package to get into the drift. And, of course, it's not hard to imagine more realistic models even though when and where failures or breaches in the waste packages would occur. So, this model uses that type of information which is developed by the waste package degradation models independently and actually looks at the timing, type of breaches, and uses that information to develop probability distributions that water will make it into a particular breach. Again, a relatively move towards realism that's not particular complicated. There are probabilities involved in the process, but essentially it's one that takes into account what we expect, more of a random type of process.

Next. Finally, the so-called bathtub effect, this is simply the issue of where a breach occurs and where water will enter in the past model and the Rev 0 models, the flow-through model that allowed for the development of a breach anywhere in the waste package and for flow-through to occur. Basically, you can have a breach and the top water can flow-through and leave the top, as well. So, there's some issues there dealing with gravity that would come into play. Now, the model is one that allows for more realism in terms of where the actual breaches develop and the timing of those breaches that allow for flow-through or for actually water to stay within the waste package. Those are just some examples of the types of changes. There are dozens and dozens of those changes. The advantage of going through Volume II of the SSPA, in every case there's a discussion of what was done in Rev 0 and the changes that occurred through the development of the new model.

Next. Well, let's take a look at what now has become familiar, the differences between the Rev 0 total system results and the SSPA results. I don't want to spend any more time related to just looking at and comparing these two things. I do want to make the point though that remember when we talked about mean values that the mean is--it's a wonderful thing. The mean is very sensitive to the uncertainty distribution. When we get to locations like this where actually there are very few realizations--let's say, out of 300 realizations, 10 of those actually are finite numbers and 290 of those are zero. That's the wonderful thing about an average. You add together 290 zeros with 10 non-zero numbers, the average is non-zero. And, of course, that's the name of the game when we deal with the small number of realizations at early times. The mean will climb up in the distribution because of the addition of a lot of zeros, zero dose. So, when you talk about means, remember that it's very sensitive to the distributions. We're moving over to where we have a much larger number of realizations, the mean and the median become closer together, and more stable presumably. Nevertheless, I do want to make some comparisons of the mean level because that's commonly where risk is compared.

Next slide. Oh, wait, before I go, what I'm going to be doing here is talking about the notion of what's changed relative to conservatism. In Volume II, there's a number of comparisons. But, I want to look at II right now. If we do some slices through this--and we could do those at various times or we could do those at various does levels, we could get a look at this wonderful thing which is the uncertainty distribution around these. And, that's really the focus of what we've tried to do and we've just really started in terms of these are system level results. It would be nice to also look at this in subsystem.

So, let's take a look at two slices. One is a slice at a particular time. We'll say the time of the peak dose which is about 300,000 years in the case of Rev 0 and we'll use 1,000,000 years since we're not sure exactly what peak does is, as we've had discussion for the SSPA.

Next slide. One of the things that we've found in this the course of this evaluation is people have different preferences for looking at this type of information. Some people like cumulative distribution functions, CDFs of this type; others like more probability distribution functions, PDS, that type. So, shown is both ways. These aren't quite PDFs, but they're close enough to make this evaluation. What's shown on these, this is a slice at peak dose time, the time of peak dose, and we have an opportunity to compare the two. What we're really looking at is the distribution of uncertainty. So, for CEFs, the slope is actually a good indicator of how broad the distribution is. We'll have two central estimates. One is the mean that's shown up here and remember the mean. Some of the problems I said before with mean and its sensitivity to outlyers. But, also we can look at the 50th percentile and compare median estimates. For those so inclined, like myself, these are the types of equivalents as PDFs.

And so, when you look at the dose at particular times, here again like I said before, we're roughly in these ball parks. We're not quite sure that's where peak dose is, but I'll show you that's the million year peak. When we compare a median to the 50th percentile, the better order of magnitude difference between these two. That's some measure of difference, potentially a measure of conservatism.

What we see is that by the additional unquantified uncertainties that have been added to this model has led to a couple of things. One is a decrease in the amplitude of the peak dose, but also a spread in--at least, measured in dose space, a spread in those levels. So, the peak dose for the revised model has a broader range to it and a lower average. And, this is going to be the theme throughout. In the Volume II, you'll see that we've done slices at various times, but that's the general observation. Because these are done on a very long time, basically the difference is the solubility models are important like we talked about before.

Next. Now, this is looking through the horsetails taking a slice at a particular dose level, a tenth of a milligram per year, and we've done that for very low doses, 10^{-5} up to 10mrem in the report. And, you can take a look at it. Again, a CDF representation and a PDF representation and then a blowup because the base-case had a number of failures within the first 100,000 years. This is just a blowup in histogram form of this first 100,000 years so you can see the nature of the distribution for the base-case. Again, the story, the same. This time in terms of the range of times to this particular dose level, much lower slope here, much broader distribution here representing presumably a broader range of uncertainty at that particular dose. In this case in some of the comparisons here, we're looking at changes that are on the order of--well, I'll show in the next slide some of the differences. But, again, what we've done here is primarily one of removing conservatisms. These dose levels, these are some of the higher dose levels. The waste package and solubility models are the most important. And, again, we have this sort of change. We talked about this before. This potential effect is one that presumably was imposed at earlier times and just maintains itself through the longer time period. And, in this case, a higher dose.

Next. So, what can we say about the significance of uncertainties and conservatisms again at a system level? We see wider ranges of doses at a given time and wider ranges of times at given doses. This is represented quantitatively by these slices through the distributions. But, we also know that from the actual evaluations at a subsystem at a process model level. There are additional uncertainties in there. We don't claim that we have them all in there, but there are clearly additional uncertainties that have been identified. Many of the models, like I went through, have been made less conservative and more realistic as they were reinterpreted.

Here are some examples and I think we probably have heard about all those through the course of the last couple of days. We see the effects. The low-temperature and high-temperature show the same effects, but they themselves are not very different in their character.

Next. So, in terms of conservatism, again these are conclusions about conservatisms made on the basis of system level results and comparison of mean estimates. After the first 10,000 years, the base case model appears to be conservative with respect to the supplemental model. I think that's fairly clear. The magnitude of the dose is lower and occurs at a later time. So, for any given time period, the dose is less indicating dose level, the time is delayed. We can look at some of the measures for the magnitude of that and, of course, it is somewhat depending on the time or the dose level itself. But, say, at 30,000 years, we're looking at orders of magnitude difference. And, for example, the time of the peak does about one order of magnitude in terms of dose.

Next. Again, this is an expression looking at the impact on the delay reached in a particular does level. When we look at this, as I mentioned before, the higher doses and the later times, that's where we start to see a little bit of separation. This is one thing to look at as we move up into the higher dose levels, say, up to a 10 mrem per year, we do see this separation that presumably was related to the temperature effects and the general corrosion rate early-on that then stayed in the system through the rest of the realizations. During the period prior to 10,000 years has been talked about. We appear to be slightly non-conservative. Since we now went from zero to a finite dose, these doses are still obviously very low with the imposition now of the new improper heat treatment model that allows for some things to happen beforehand.

Next. I want to talk just a minute here about where we go from here? What are the types of things we're going to be doing and are doing right now to look at the implications? There will be further comparisons of the system level types of analyses and one-off sensitivity analyses, but also some of the types of things that might show before; to look at residence time within particular zones. We're talking here about delays. What are the components of those delays; in particular, dose levels, for example, and other types of conditional assessments that are similar to that. The other part of this, too, is what are the contributors? It's common in a risk analysis to talk about, say, the 5th to 95th percentile spread in uncertainty. What are the contributors to that from an uncertainty point of view? We've talked mostly about what contributes to the mean, but what contributes to the spread? Those are some of the types of analyses and contributions that we're looking at now. And, finally, an issue that's been raised before of what about robustness? What's the likelihood of change in these models in the future? It's something that we need to look at.

Next. Communication of uncertainty has been talked about before. There was quite a bit of discussion at the last Board meeting and I wanted to be sure that it's up here. It's clear that every time we have discussions or interface with groups trying to present and slice and dice this information that there's always a need to come up with better tools for that communication so people can understand the implications and communicate it. You remember at the last meeting, Dr. Cohon asked Bill Boyle how would you summarize this in a three-page letter to the Secretary of Energy? I can't remember what Bill said, but it was a really good answer. We need to have those tools for that type of communication. And, finally, development of a guidance for future uncertainty treatment. We've learned a lot in this review of uncertainty and we need to be consistent with the licensing strategy. We tried to deal with things like bounds, what are bounds, should we use them, when and where, how should we quantify uncertainty, and so on.

And, the last slide. We need to illustrate how uncertainties can be quantified. What types of tools do we have? As I mentioned before there's a range of familiarity with tools of probability and uncertainty quantification and how do we document how we've done it and finally outline our approaches to how we'll communicate uncertainty characterization in the future.

CHRISTENSEN: Kevin, thank you. I think, given our time, we'll not take any questions and we'll maybe come back. So, we will move to the next speaker, and then if we have enough time, we'll address the entire thing.

Gerald Gordon who is with Bechtel SAIC who will be talking about an evaluation of corrosion process.

GORDON: Good afternoon. These are the elements I'd like to cover over the next 15 or 20 minutes depending on how much time I have. These are various corrosion elements and associated mechanisms. I'll cover each of them, but in a fairly brief manner.

Next slide. In terms of establishing the long-term corrosion rates, as you've heard, the current corrosion rates are based on weight loss measurements from the long-term corrosion test facility and the maximum exposure time that's been evaluated is a little over two years. Because the rates are very low and the measurement uncertainty in doing weight loss measurements with these low rates is relatively large, it limits the ability to determine small changes. Also, the temperature range is limited to 60 and 90 degrees Centigrade.

That combination doesn't allow us to back out at this time the temperature or environmental dependencies. They're all within the uncertainty. As a result, we have Fulton experimental and modeling program underway to corroborate these measured corrosion rates, as well as to establish the temperature and environmental dependencies and to provide a basis, a deterministic basis, to allow this short-term data to be extrapolated over time.

What I'd like to briefly cover are electrochemical and microscopic techniques and results that tend to corroborate the long-term corrosion test facility results. I'll cover results from the project, as well as some corroborative results, from the literature. What we have here are two of the principal electrochemical techniques that we're employing that have higher resolution capabilities to measure corrosion rates. This plot on the left is a linear polarization plot of corrosion rate versus exposure time over about five months exposure. In this case, these data were generated at 25 degrees Centigrade and slightly concentrated J-13 water. And, the mean rate of about .01 microns per year, interestingly, corresponds to the 25 degree C rate that Greg Gdowski showed you earlier where we've now incorporated a temperature dependency. And, CDF curve for 26 degrees C, the median corrosion rate is about .01 microns per year. On the right is another technique, potentiostatic polarization technique. where one applies a fixed potential and measures the current density which in (inaudible) solution tends to relate to the corrosion rate as a function of time. And, these are data generated at Southwest Research Institute on NRC work in what I'll call the relatively aggressive environment because it doesn't contain nitrate and sulfate or, at most, only millimolar concentrations. But, we see the corrosion rate tends to decrease with time and in about 100 hours or so it reaches a steady state value. We're using both of these techniques to obtain corrosion rate measurements as a function of time, environment, applied potential, metallurgical condition, and I should add temperature.

Next slide. It's important in extrapolating the corrosion performance of Alloy 22 over time to understand the corrosion mechanism. And, one part of that is to understand the corrosion films that form on the surface compositionally thickness-wise and how the kinetics of film growth occur. If you look at the literature on Ni-Cr-Mo alloy corrosion films, they tend to consist of at least two layers of very thin inner layer next to the metal that is on the order of 10 to 100 angstroms thick and it tends to be Cr_2O_3 which based on thermodynamic data is the thermodynamically stable phase. And, this layer also contains molybdenum, nickel and in the case of Alloy 22, a small amount of tungsten. And then, outside of that inner layer, there tends to be a less protective layer where the metal has dissolved and re-precipitated on the surface and that can be an oxide, hydroxide, oxyhydroxide, or some combination. We're starting now to characterize the passive films on Alloy 22 rather than generic Ni-Cr-Mo alloys. These are the techniques that we have started to use and I'll describe briefly some of the results from these various techniques.

Next slide. You've seen atomic force microscopy photos of the surface of some of the corrosion coupons at previous Board meetings. In this case, I've picked a coupon that in examination, after taking it out of the tank and ultrasonically cleaning it, had very little scale on the surface. Often, one tends to see silica or sodium chloride scales on the surface. This sample is very clean. On the left is a control sample and this sample was exposed for one year in 90 degrees C simulated concentrated water. And, the samples are polished through 600 grit paper before they're put in the tanks and you can see the polishing scratches on the surface and they tend to still remain very sharp, although that's not a quantitative measurement of corrosion rate. We don't see any localized corrosion. When we compare the sharpness of the surface with the measured corrosion rates by descaled weight loss, they tend to be less than 60nm of metal loss. The image is consistent with that, although it's not a quantitative measure.

The next slide. To get a more quantitative measure of film thickness, Lawrence Livermore Lab has started employing what is called the tunneling atomic force microscope. It's a technique where you apply a biasing voltage with a contacting point on the surface of the sample and you measure the resulting current which tends to be an electron tunneling current. This technique is very sensitive to changes in the electrical properties of the oxide and can also detect very small variations as you raster across the surface. It's a technique that more recently has been used in the semi-conductor industry and it's capable of detecting very small changes in the oxide thickness.

On the next slide is a tunneling AFM image compared to a conventional atomic force microscope image. This is on an Alloy 22 sample exposed for 45 days in 200 degrees C air. And, we're really seeing an electron current image of the surface and these lines are scratches on the surface, very, very fine scratches. The reason we're getting a contrast is the oxide tends to be somewhat thicker on the scratched areas. Looking just at the surface topography with atomic force microscopy, we can't resolve that detailed structure.

The next slide, this plot on the left is a plot developed by exposing a sample in air 200 degrees Centigrade and monitoring the change in thickness over time. What's plotted here is the applied bias voltage and the resulting current and the solid lines are calculated lines for different oxide thicknesses, one, two, three, and four nanometers, and the data points are experimental data. The film starts out with a passive film air exposed on the order of 2nm, and after 28 days, it's approximately 2.8nm; after 45 days, it's grown somewhat to 3.2. Data had just become available at 120 days and there's very little, if any, increase in thickness beyond the 45 day measurements indicating the film is tending to reach a constant value. We've started out in air, but we do plan to go later this fiscal year into testing in a range of aqueous environments over a range of conditions and obtaining the kinetics as a function of environment and temperature, and also can do that as a function of applied potential. On the right is an analysis of the passive film on Alloy 22 by time of flight secondary ion mass spectrometry. With this technique, you basically can spatter an inert gas, such as argon, across the surface and you spatter away one atom layer at a time and you analyze the atom layer using mass spectrometry. So, you can determine the composition of the film as a function of depth on a very fine scale. These are data generated at the University of Western Ontario in a fairly aggressive one molar sodium chloride at a very acidic pH and 85 degrees Centigrade. The upper right plot here is Alloy 22 with the initial air grown oxide film. What you can see is the form is very rich in molybdenum and chromium. They tend to concentrate near the very outer surface and the thickness of the film is on the order of 3nm which is very similar to this starting film of 2nm. When the film is potentiostatically polarized to a given potential--in this case, 200mV which is slightly above the corrosion potential in this solution--we see a very similar film to the air formed film with molybdenum and chromium enriched near the outer surface and some evidence of nickel, also. When we go up to a pretty oxidizing potential, the chromium starts to drop relative to the molybdenum. And, we know from x-ray photo electron spectrometry measurements which are in the backup slides--I don't have time to go into them--but the valence state of chromium in this film is +3. What is very likely to happen in here at this high potential is we're oxidizing the chromium +3 which is insoluble to chromium +6 which dissolves into the water. And so, the chromium concentration is decreasing, especially near the surface of the film.

Next slide. In extrapolating short-term data or relatively short-term data at very long times, it's important to understand any potential degradation mechanisms that might occur over long times that could degrade the protectiveness of the passive layer. These are some mechanisms that have been speculated about in terms of their applicability to

Alloy 22. There are undoubtedly other mechanisms, as well. We do have an active plan to address these issues with very focused tests. The probability, we feel, is low that these mechanisms will actually occur over time, but it's important to get some handle on the probabilities. So, focused tests are the way to do that. We do have some tests underway and we have test plans and are developing more to be able to address these issues. In the backup slides, there's more detail on each of these particular mechanisms and in some cases our current plans to address understanding those mechanisms.

Next slide. As you know, the project has convened an International Waste Package Performance Peer Review Panel. The first meeting was held in Las Vegas a month or so ago. And, the panel will, among other things, address the appropriateness of our plans and our current path forward efforts. As you're well-aware, the Board is also convening in this case a workshop on July 19 and 20 to address these same degradation mechanism issues.

Next slide. In attempting to project relatively short-term data, in addition to our own data, there are industrial data on the corrosion behavior of Ni-Cr alloys that go back almost 100 years to some of the first alloys. But, that's still a short time, and in attempting to project forward, a deterministic approach offers the potential to gain insights and to reduce uncertainties associated with our current empirical extrapolation. When one looks at mechanistic models to describe passive film growth behavior, the two main models are the point defect model and the semi-conductive oxide model. There's significantly more data in the literature on the point defect model in a range of alloy systems, as was described earlier by Greg Gdowski. We are currently developing a point defect based generalized corrosion model for predicting cumulative general corrosion damage. And, it will predict both general corrosion and ultimately breakdown of the film if it were to occur in localized corrosion. And, again, the details of our current status of that model and the path forward is in the backup charts.

Localized corrosion is if the passive film breaks down and we get localized corrosion, the rates tend to be orders of magnitude higher. So, it's very important to understand the margins that exist against localized corrosion occurring. In considering that, it's important to know that the range of relevant environments are buffered or inhibited, if you will, with nitrate, sulfate, probably carbonate and silicate. This anion ratio of chloride to nitrate plus sulfate for all of the relevant environments that have been identified tends to lie 2 to 1 or less. There's a significant amount of buffer ions to chloride ions. In addition, the corrosion potentials that are measured relative to the critical potentials, they're significant margin and we've described some of that data to the Board earlier. However, it is possible if we're very oxidizng, if the potential is very high, and we're in concentrated chloride solutions where the chloride to nitrate and sulfate tends to be high and high is on the order of 10 to 1 or higher. One can break down the passive film, especially with a crevice sample, and polarize the very high potential.

Next slide. What are shown here are some data out of the waste package degradation PMR of the measured corrosion potentials measured during short-term cyclic polarization tests over the range of environments, the significant range of environments, and they tend to lie from about -250 to -50mV on the silver-silver chloride scale. And, over the range of the long-term corrosion test facility of roughly to 60 to 90 degrees and, in fact, over the whole range, there's really--the slope of the temperature dependency curve is relatively low when one considers comparing that to it. In this case, this is a plot generated an University of Virginia on a project program of the crevice repassivation potential. These are crevice samples polarized at different potentials or actually cyclically polarized and the repassivation potential is measured in this case in 4 molar lithium chloride with an anion ratio of 10 to 1. So, it represents a pretty aggressive environment. And, we can see that the repassivation potential which is the minimum potential for localized corrosion tends to increase very rapidly as we go from about 95C to about 85 Centigrade on the order of 600mV of increase. And, when one compares the corrosion potential with this repassivation potential, the lower the temperature, the more margin, and the margin tends to increase rapidly in this temperature range.

Next slide. This is a comparison of Alloy 22 corrosion behavior measured potentiostatically with a number of other--it's probably hard to read that. But, Alloys 625, C-4, C-276, C-2000 compared to Alloy 22. These are all corrosion resistent Ni-Cr-Mo alloys. And, what we see over this temperature range from this, we're dealing from 25 Centigrade to 85 Centigrade, is that there's very little temperature dependency of Alloy 22. There's some, but it's small at 200mV which is near the corrosion potential. When we go to a pretty oxidizing potential, the corrosion rate slope goes up, but it's still not large compared to some of these other alloys. These take off of these vertical lines where crevice corrosion is observed. At this temperature and potential, crevice corrosion starts. It doesn't occur on Alloy 22. It does on some of the other alloys. We have data intermediate potential of 350mV which we've used to calculate an activation energy and it's in the SSPA. I think Greg

Gdowski, it was on his chart earlier. This activation energy of 32kJ/mole is very close to the 36kJ/mole for the University of Virginia data.

Next slide. So, from the very brief overview I gave, I think we can conclude a number of different points. One, both the project and the literature results, as well as other multiple lines of evidence, such as the commercial analogues and also potentially the Josephenite that I described at a pervious Board meeting, they support the basis for extrapolation of the corrosion rate over long times. In terms of localized corrosion, the currently measured resistance to localized corrosion is very high, but one does gain margin, very significant potential margin, with lower temperatures. We do have a very comprehensive experimental and passive film modeling program. It's defined and it's underway. The data generated will decrease uncertainty. By the end of this year, we'll have extensive data, but the program goes on longer term.

Next slide. As I mentioned, we did convene a peer review panel to assess the adequacy of our current approach and path forward, particularly in the areas of extrapolation of corrosion rate data over long time periods, long-term passive film stability, and our degree of confidence in the localized corrosion margin, if you will, over long times.

Thank you.

CHRISTENSEN: Thank you. We have time for a couple of questions. Sagüés?

SAGÜÉS: Yes, thank you. This question goes back to Greg Gdowski's presentation this morning and one of the issues that you mentioned which is the introduction of the temperature dependence of the uniform corrosion rate. I know that the effective value for an activation (inaudible) was obtained from the potentiostatic experiments and some of the other potential dynamic experiments that were performed at University of Virginia by John Scully and those people. Now, for the purposes of the SSPA calculations, you took that activation energy and applied it to which uniform corrosion data?

GORDON: The data from the long-term corrosion test facility.

SAGÜÉS: Okay. And, of course, that one involved a number of temperatures. I understand that those data didn't--the results didn't change very much with temperature.

GORDON: Right.

SAGÜÉS: So, I guess, what temperature center point did you use to--

GORDON: 60 degrees C.

SAGÜÉS: 60 degrees. So, you took the lowest of the temperatures that you had before and then you went from that temperature up and down. Is that correct?

GORDON: That's correct. So, the rates tend to be lower at low temperatures than the previous rates which we took essentially a constant rate over temperature before and they tend to be higher at higher temperatures than we used previously in TSPA/SR.

SAGÜÉS: Okay. That's really it. Considering the time that we have available, I'm going to stop.

CHRISTENSEN: Okay. I think given our time what we'll do is take a brief break. Carl Di Bella?

DI BELLA: Carl Di Bella, thank you. Jerry, I notice all of the data--the aqueous corrosion data in your paper here is at 85 degrees--no, 90 degrees Centigrade except for, I think, one or two points at 120. But, this morning, it was made clear that aqueous environments on the waste package can exist from 120 to 160 or so. What are you going to do to get some data in that temperature range?

GORDON: We're doing several things. We're running a series of potentiostatic tests to measure corrosion rate over a much broader range of temperatures and environments and we're running cyclic polarization tests. I think the last of my backup slides are some results in calcium chloride with and without nitrate present. You have my backup slides and it's the very last slide.

(Pause.)

GORDON: This is a cyclic polarization curve. It's at

120 degrees Centigrade. We are developing data at higher temperatures. But, what is shows, this is basically saturated calcium chloride, in this case with calcium nitrate present and in this case only calcium chloride. And, what one sees is that the margin between the corrosion potential and what is very likely in this case a critical potential or breakdown potential is relatively small, as you might expect in pure calcium chloride. Magnesium chloride would be very similar. But, with the nitrate present, the curve basically traces many of the other curves that we've generated in the other simulated concentrated water kind of environment where the curve starts to break over probably from oxygen evolution rather than film breakdown because we've seen no evidence of localized corrosion on the sample after the test. So, we've gone to over 1000mV in this case without localized corrosion. But, if the nitrates and the sulfates, presumably, and some of the other buffer ions aren't present, these environments would be very aggressive.

DI BELLA: I think that chart is very illustrative of why it's very important for the project to show that the buffer ions are always going to be there.

CHRISTENSEN: Carl, thank you. I want to propose a very brief break, five minutes, mainly for the benefit of middle-aged men in this room, such as myself. We will reassemble in five minutes.

(Whereupon, a brief recess was taken.)

CHRISTENSEN: We do need to move along and I apologize for all my colleagues here that we're having to be on such a relatively short timetable.

Our next speaker is James Blink from the Lawrence Livermore National Laboratory who will be giving us an evaluation of the range of thermal operating modes. Jim?

BLINK: I'm the lucky one who gets to summarize all the information that was presented in the earlier talks just trying to contrast the higher versus the lower temperature modes. So, I've got the synthesis job in a pretty short time. The way I'm going to do it is to walk through the different aspects of the system following the water droplet like we usually do and I'll try to show you the basic data or the things that are dependent on temperature at the process level and then I'll try to draw some conclusions as to whether it makes a difference at that process level for that subsystem based on the status of our current models. Leon in the dry run asked me if I would try to draw that conclusion. So, I did and I put it in blue italics for Leon.

Next. The first area is the thermal seepage. There's a small difference in the early times in the rate of seepage between the higher and the lower temperature operating mode. And, there's also a small difference in the fraction of locations that see seepage, but those differences are not very large and don't make much difference to the overall TSPA model. The differences in this time frame are related to the humidity issue that we discussed earlier and I'll get into that a little bit later.

Next. Actually, can you go back one, sorry? The model, as Bo pointed out to you for the low temperature operating mode is essentially an ambient seepage model, but for the high temperature operating mode, the process model gives you lower seepage than the TSPA abstraction and that is, in turn, lower than an ambient situation. So, have more conservatism in the TSPA than we do in the process model.

Next. The thermal history, the question is whether our models, our PA models and our process models, apply equally to both operating modes. What we did here is we took the lower temperature operating mode thermal history and we just started adding times to it delaying it in time until we could see it lay pretty close to the high temperature operating mode late time period. And, the delay, depending on whether you're trying to match the peak or match most of the curve, is somewhere in the neighborhood of a couple thousand years to 5,000 years. The point being here is that if there aren't any permanent changes made to the natural system or if you haven't done anything to the engineered system to fail it during the thermal pulse here, when you get out in the later time of the high-temperature operating mode, it's just like the low-temperature mode. Or to be said another way, the models are equally applicable and we just have to add on top of the lower temperature models the information that happens during the high temperature pulse.

Next. This is a slide that Bob MacKinnon showed you earlier. It shows you the range of temperatures that one would get from different types of waste packages for the higher and the lower or from different locations in the repository for a typical waste package for the higher and the lower. The variability range by location and type is on the order of 20 degrees, but the variability range between operating modes is of the order of 90 degrees. So, clearly, operating mode makes more difference than location or waste package type.

Next. This is a slide you haven't seen. I wanted to show you a few that you hadn't seen before. This shows four snapshots in time; right after closure, at the peak temperature time for the higher temperature operating mode and the lower temperature operating mode, and then at 2000 years and 10,000 years for each. This shows the temperature across the footprint for a typical waste package, a typical PWR. What I've done is I've changed the rainbow, the color scale, as I go along in time, but I've made the scale be the same scale for the higher and the lower temperature operating mode at each time point. So, you can see that at the early times near closure, of course, the high-temperature operating mode is much warmer than the low. And, similarly, at the peak temperature, it's much warmer. But, as we go in time, they get closer and closer together visually and have the same sort of pattern of the cooler temperatures eating their way in from the edges. The spatial variability at almost all the times is fairly similar, as well; just the number of degrees from the edge to the center. At 10,000 years, although the distribution is similar, there is a slightly cooler, only a few degrees, but it's slightly cooler, and that's the source of that relevant humidity difference. It's just a few degrees, but that changes the heat transfer rate just enough to cause a humidity depression still at 10,000 years.

Next. This one shows you the humidities now. A couple of things that just jump right out at you, we're dryer in the high-temperature operating mode in these two periods and we're dryer in the low-temperature operating mode at the later periods. And, Bob MacKinnon showed you where the two curves crossed. This is a graphical way of looking at it. If you study these figures closely, you can also see the edges and center are a little bit different from each other. You have a drier high-temperature operating mode early because you're at a different point in time. Mike Wilson told you the closure time is 50 years and 300 years for the two cases and that makes a difference because the waste package heat output is different at those two times. Similarly, the high-temperature operating mode dries out some of the near-field rock and so the relative humidity is based on a different rock saturation as a peg point. But, when you get out into the 2000 years to 10,000 year time frame and the rock is rewet, now it's just the Delta T, the temperature difference between the inside and the outside of the drift.

Next. This shows you a--it has a probability distribution. This augments the one that Bob MacKinnon showed you. The left side is showing you the time it takes a waste package to come back to 80 percent humidity, the time coming back from the dry side. And, this shows you the waste package temperature at that point in time. A couple things are clear from this. First of all, the low-temperature operating mode does stay dry longer just as we've shown you before and you can also see the ranges. The three curves in each of these are the uncertainty band based on the uncertainty and the infiltration rate coming into the top of the model. The lower temperature operating mode humidity does last--stay depressed longer. This formula down here should answer the question that Dan asked a couple of times. The relative humidity is a ratio. The numerator is the same in both of these because we have a well-mixed gas in the drift, but the denominator, the saturation pressure, is a strong function of temperature and that slight difference in temperature between the two will drive the humidity.

Next. This one is sort of a digression from the higher versus the lower. Our base case for the lower temperature operating mode is 300 years of ventilation, 1.1 meter average spacing of the waste packages, but with variable ranging from .1 meters up to between 2 and 3 meters and 15 cubic meters per second of ventilation. That's one way to get at a temperature on the waste package of around 85 degrees C peak. But, there are other ways that one can get there that our models were able to distinguish. Another way that we looked at is we pushed the waste packages closer together, just as in the high-temperature operating mode. So, they're about 10 centimeters apart. But, we took the hottest waste packages and we reduced their heat level. And, we did that by taking some assemblies out of those hot waste packages by getting the PWRs down to around 16 assemblies apiece. And, that made a much smoother heat distribution along the line of the drift and we achieved about the same peak temperature. A third way to do it is we put the full capacity waste packages together so that the drift looks identical to the high-temperature operating mode drift; the same waste packages, the same spacing. But, we pushed just farther apart to about 97 meters apart. All three of these have about the same peak temperature and they have exactly the same footprint, same aerial mass loading. So, it didn't matter much which way we went from the viewpoint of temperature. Given that, then I would say other factors besides temperature or dose, factors such as uncertainty or worker safety or cost, could be very important to the decision maker to decide just what design we would go with if we were going to go to a low-temperature operating mode. In the backup slides, I've got two of three more on this subject including one that shows what we call the ball park chart.

Next. This is also one that Bob MacKinnon showed you. This one has had a few updates to it because it's a little more recent than his. What we are showing here is on the Y axis is the peak temperature normalized to the base-case. Each of these bars varies in independent parameter that is either uncertain or has some spatial variability within the mountain. The one that's most important to us is the lithophysal porosity. 12.5 percent is our base-case and that's taken from mapping data of the cross-drift in the ESF. But within that area, there's regions that have almost no porosity, lithophysal porosity, and regions that are up to 25 percent. So, we went ahead and recalculated the thermal conductivity and the heat capacity based on that lithophysal porosity and then stuck it back in the model to see what temperatures we got. When the lithophysal porosity was high, we could get 80 degrees C higher temperature than the base-case. When the lithophysal porosity was low, we could get about 15 degrees C depression in temperature or lower temperature. For the lower temperature operating mode, we did the same thing and now the range was about -5 degrees/+20 degrees.

You can see from the next one that the thermal conductivity is the biggest part of that, the heat capacity being a much smaller part of that sensitivity. We went through and we did this thing for looking at the bulk permeability of the rock, the variation we could have in that, the thermal conductivity of the invert, the variability amongst the waste packages. How does it vary from the coldest defense high-level waste package to the hottest PWR package? We looked at the way we treated the in-drift heat transfer. Did we do thermal radiation using a T^4 kind of law and calculated it explicitly or do we go and take a handbook correlation and use it?

Similarly, in-drift air permeability in these models, we varied that. Ventilation efficiency, plus or minus 10 percent. It made a small difference, but not a very large one. In our calculations, we assume all the waste goes into the mountain at the same time, 300 years for the low-temperature operating mode. But, actually, the emplacement period is 22 or 23 years in base-case for these calculations. So, we went ahead and varied that period plus or minus 22 years and it made almost no difference for the low-temperature operating mode.

The other one that was a very large difference was the ventilation efficiency, how we treated that. In our calculations of ventilation efficiency, what we actually get is an efficiency versus time. Starting out very low and then going up sort of like a nose and then finally leveling off towards an (inaudible) or slowly increasing with time. Normally, the way we use this result is we average it over the ventilation time and just reduce the power of the waste packages in the calculation to avoid having to do the Napier/ Stokes kind of ventilation calculation simultaneous with the heat transfer in the rock. For this calculation, we went ahead and put it in as a time dependent function to see what would happen. And, we had preclosure temperature spikes in that calculation. For the high-temperature operating mode, it got almost to the level of the post-closure temperature; and, for the low-temperature operating mode, it actually got up to much higher than the post-closure temperature in the neighborhood of 120 degrees for a preclosure temperature. Now, is that the correct answer? No, because the ventilation calculation itself has some assumptions built into it, but what it tells us is we've got to go pay more attention to that and calculate this on our next round even more carefully and couple the two models together there.

The last one, those are the three methods of achieving the low-temperature operating mode.

Next. Bo showed you this one. This is the thermo-mechanical, the hydrological changes to the mountain. There's a low-temperature and a high-temperature case and you can see that the changes are about the same. The variability in the fracture permeability spatially in the mountain is larger than the kind of changes we would induce either temporarily or permanently. So, it doesn't make much difference to the performance, either th seepage or the transport.

Next. This is the chemistry. Bo showed you this one, as well; the carbon dioxide, the pH, the chloride, and the fluoride for the ambient situation and then for the higher and lower temperature operating modes. The biggest feature of this is the depression in the CO_2 in the gas and the reason for that is because we're displacing almost all of the air which includes the CO_2 for a while and replacing it with water vapor.

Next. This one shows you the pH up here for the high-temperature and lower temperature operating modes and the CO_2 for the same two modes as a function of time. Now, this is what it looks like when it comes out of the rock into the drift. This is taking that more detailed curve from the previous chart and abstracting it in time. So, it has this histogram kind of nature so that they can put it into the TSPA model. Before it actually is used in the TSPA model, however, we put it together in an equilibration model, a precipitation and salts model, and Bob MacKinnon showed you the results of that. So, I want to emphasize at this point in time, the gas composition has not been equilibrated with the liquid composition. It's an intermediate step in the process.

Next. Okay. Waste package corrosion, you've heard that from three different speakers already. This is my bottom line of it. This is the curve that Jerry and Greg showed you, the general corrosion rate as a function of time with the 25 to 75 percentile uncertainties. That was calculated from the potentiostatic polarization measurements in aggressive environments in order to get corrosion fairly quickly. Then, we took that temperature dependence and applied it to the 60 degree C long-term corrosion facility data so that the rate would go higher than measured at 90 and go lower at the 25 degrees C. When you put that into the model, you only have some differences at the beginning and out here. In effect, this difference right here is the cause of that difference in the dose curves that Mike Wilson showed you. In here, you have the higher temperature and the difference in time that the corrosion begins because of the closure time and the humidity depression for the lower. Out here, you have the history of the corrosion at the higher temperature having caused your waste package to fail a little bit sooner.

There's a couple of other things that we add to this general corrosion rate. For microbiological induced corrosion, we add a factor between 1 and 2 to that corrosion rate. And, for aging for the closure well, we add a factor of between 1 and 2-1/2. The bottom line though is the high and low-temperature operating modes are fairly similar unless, of course, this delay is something that would cause you to make a choice.

Next. I've tried to synthesize the local corrosion into one graph. This one takes a little bit to follow. The temperature and humidity curves that Bob MacKinnon and I have showed you, I've cross-plotted them so that at the time of peak temperature and maximum dryout, you're down here. And, then in time, it proceeds up eventually at 100,000 to a 1,000,000 years and you're up here back to near 100 percent humidity and 25 degrees C, the ambient temperature. The width of this band is the different locations on the footprint and the different types of waste packages. So, that band represents 7,000 individual waste package temperature and humidity histories. Similar for the blue band, it does the same thing for the lower temperature operating mode. So, time on this scale is going upwards and to the left.

I've superimposed on this two-thirds of the crevice corrosion initiation window of susceptibility. That depends on what kind of salts you have that determine the chemistry of the film. If mag chloride alone was determining it--this is the deliquescence curve that Greg Gdowski showed you for mag chloride, this is the deliquescence curve for sodium chloride up here. So, for a mag chloride situation, the temperature boundary is in the 85 to 90 degree C range. Of course, you can't get above the boiling point and this whole window here is a window where it might happen if the pH constraint is also met. For sodium chloride, the window is much smaller. If you have a buffered situation, it may be that there is no window, at all. Maybe that the buffering ions counteract the chloride ions. There is an error on this chart. These two toes should be together. We extrapolated these two curves off of Greg's chart and the artist didn't realize that they had to end up together. Bottom line is in the process model, we look at temperature, we look at chloride, and we look at pH. The pH dependence was much stronger than either the temperature or the chloride.

In TSPA then, we made a simplified abstracted model where that strongest contributor, pH, was the dominant one. All right. The result was both operating modes had no crevice corrosion. In the low-temperature operating mode, the temperature criteria alone at the process level would be enough to conclude you wouldn't have the crevice corrosion. But, at the high-temperature operating mode, you have to do to the pH criteria which is a more complicated argument. A long story as to why there was no difference in the end that the crevice corrosion didn't cause you a problem. In a sense, what that means is we're not talking about performance here for localized corrosion, but rather uncertainty in the conclusion.

Next. Water diversion in the EBS. We've done some improvements in the model. Kevin Coppersmith summarized them quite well. We've changed the way that we treat the geometry here so water doesn't flow uphill or seek a hole, but is properly distributed. And, that improved our basic TSPA model. We also did a one-off study in the unquantified uncertainties area where we had condensation occur on the inside of the drip shield. And, we allowed that condensation to occur anytime that the drip shield was cooler than the invert. And, we made the model water available, but be controlled by the amount of water that was evaporating from the invert which we are calculating in the TH model.

And, we had one more thing we had to know; if water condenses on the surface, what's the probability that it will drip on the waste package as opposed to running down the side in a flow? And, because we didn't have a clear model for that yet, we sampled between zero and 1 for that; zero meaning that it would all flow down the side of the drip shield and 1 meaning it would all drip. And, we sampled the continuous range in between that.

One last thing is we looked for corroboration of that model to see if we could get at that sampling and we had one 25 percent scale test out at the Atlas facility. In that test, we did have some condensation. However, it all occurred down here on the invert. So, we didn't actually get to determine whether it was a dripping phenomenon or a film flow phenomenon from that particular test.

Next. Waste form, I've plotted here the two things that were modeled that were found to be the most important that were temperature sensitive. One was the neptunium solubility, 25, 60, and 90 degree C curves here. The other had to do with the cladding. This is the corrosion rate or oxidation rate of the spent nuclear fuel through a pinhole in the cladding and this is the resultant unzipping rate of the cladding. All of the other items in the bullet were looked--the bullets were looked at, but we didn't put them into the model either because we didn't know them well enough to put them in the model or because we knew them well enough to know that they were a fairly small factor.

Next. Engineered barrier system transport. We've also spoken about that earlier today and yesterday, both in the waste package and also in the invert. This shows you the normalized diffusion coefficient normalized to 1, being the late time values for the high and the lower temperature operating mode. Because these times are the times of maximum temperature, 1,000 years, 10,000 years and so forth and those times have different temperatures for the different operating modes, the temperature dependence is shown here as the diffusion coefficient dependence. When you apply these to a breakthrough model, this is the breakthrough curve for the two. It's, in essence, a transient time across the invert. Although it looks fairly different, the log scale can fool you. It's only decades to a century or so and that's probably not enough to make much difference.

Next. The unsaturated zone transport, we've talked about the drift shadow. I wanted to focus here on something that gets brought up every once in a while, usually when we're at one of the KTI meetings with the NRC. That's will there be any temperature-driven changes to the Calico Hills area that would either cause the flow through the Calico Hills to be different or would cause the sorption in the Calico Hills to be different? And, the conclusion from the UZ people's work was that even for the high-temperature operating mode, the temperatures only got up to about 75 degrees C. That wasn't high enough and it didn't persist long enough to make significant alteration that would change sorption or flow.

Next. You've seen this one before, the total dose, and we've explained why there's small differences here and larger difference here. Most of the failures happen well-beyond the thermal pulse and so the dose rates are generally similar and we understand why they are a little bit different in some places.

Next. This is the horsetail plots for the two. You've seen those, as well, before. If I mixed up the labels on those two, I'm not sure anybody would have noticed. They just look very, very similar. My summary of this whole putting all of the process models together is the TSPA uncertainty ranges for the two modes were similar. The models, the same models, could be applied to both operating modes. The process level models were used to evaluate the subsystem uncertainties and in some cases those were propagated into the TSPA abstractions, but in other cases, they weren't. And, I think that's the last slide other than the backups.

CHRISTENSEN: Thank you. Time for just a couple questions. Paul Craig and then Dan Bullen.

CRAIG: One of the main issues that we've been talking about over the years is coupled processes and the temperature dependence of coupled processes. And, when you find these small differences, one wonders about the modeling of the coupled processes. I recall that a year or so back, we had a presentation from Bill Glassley of Livermore who had done three-dimensional computations using the supercomputer. And, he talked a lot about the possibility of dissolution and precipitation and he also had many chemical species that did have strong temperature dependence to the reactions. So, I just ask--I have no idea what the answer is--but if one were to go to that kind of a consideration of how the coupled processes operate, isn't it possible that you would find much stronger differences between the high-temperature operating mode and the low-temperature operating mode? I don't expect you to know the answer to that. I'm just laying it out as a speculation to try and understand why the differences are so small in all the presentations that we've heard here and just suggest that in reality the differences might be much larger.

BLINK: Yeah. Bill Glassley's model is a model that's a step forward from the models we're using in that he can calculate a bigger domain, a bigger physical domain, and he can put more reactions in.

CRAIG: Yeah.

BLINK: But, for the minerals that seem to matter the most in either his calculation or the calculations that Eric Sonenthal does at Berkeley, we get very similar answers. Bill had some answers four or five years ago that showed more reduction in permeability than our current calculations and those are attributed to a change in the fracture porosity of about a factor of 30 from that point in time to what we believe it is over the last few years with our best knowledge of the system. Based on that best knowledge of the system, that 1 percent of the fracture porosity could get reduced to 99 percent of the 1 percent. So, it's a very small change in the calculations. I don't think Bill's calculations are much different than that for the repository situation.

CRAIG: Okay, thank you.

BULLEN: Bullen, Board. Could you go to Slide 14, please? I have a question about your MIC. It appears that you're adding MIC or a factor to multiply for microbiologically influenced corrosion to the general corrosion mode. Why is that instead of the localized corrosion mode?

BLINK: That's really an excellent question because I would think mechanistically that the MIC would break down the passive film and be the initiating point. It's really an empirical result. Joann Horn did the coupon tests and the morphology she saw was a relatively broad attack. So, that's why they applied it. I suspect that when we do more work on this over bigger samples that it might, indeed, end up being a localized effect.

BULLEN: Bullen, Board. Is that in one of the chapters of the SSPA or is there an AMR or PMR that I could go dig back through?

BLINK: That's actually, I think, in the--it's in the waste package PMR. Jerry, do you know which AMR it's in or--

BULLEN: That's okay. It's buried in the AMR and PMRs; it's not--

BLINK: Yes.

BULLEN: Okay.

BLINK: Yeah, that was in the TSPA/SR. It hasn't changed, I don't think, for the SSPA.

SPEAKER: Alloy 22 AMR.

BLINK: Say it again?

BULLEN: Alloy 22 AMR, okay.

BLINK: Alloy 22 general corrosion AMR.

BULLEN: Okay. One more quick question. Can we see Slide 21? And, just so Kevin Coppersmith doesn't get the right idea that if he talks long enough, he doesn't get any questions, I'm going to ask him this question that I was going to ask him before our esteemed chairman cut me off, a quick one. This one, Jim shows us and says we don't see any difference between LTOM and HTOM. The question I have is that in evaluating an additional work for the unquantified uncertainties, do you expect to see a differentiation between the two; if so, why, and if not, why not?

COPPERSMITH: This is Kevin Coppersmith. Let me get clarification. Are you talking about the evaluations that

are going on right now and will in the next couple months or
are you talking about in the years in the future?

BULLEN: Bullen, Board. First, in the next couple months because we want to know if there's going to be a difference between now and SR and then you can speculate about long-term, if you want, but basically the next couple months was my question.

COPPERSMITH: No, I think over the next couple of months, the goal is to gain additional insights into where we are on these right now. What are the contributors to resemble the median and mean differences and so on that Jeff Wong talked about earlier, the uncertainties as a function of time, function of does, that type of thing.

BULLEN: Thank you.

CHRISTENSEN: Thank you, Jim. Our next speaker is Ardyth Simmons who will be talking to us about the development of multiple lines of evidence. Ardyth is with Lawrence Livermore National Laboratory.

SIMMONS: Well, this is going to take us slightly to return from the highly technical talks that you just heard. Of course, we spoke to you about multiple lines of evidence in the April meeting and also in the May meeting. You've heard about them sprinkled through all the talks today pretty much. So, my presentation will have more of the flavor of how we developed the multiple lines of evidence. I just want to take you back to what we've been using as a working definition and that is that multiple lines of evidence are independent lines of reasoning that are used to demonstrate how well a system, a subsystem, or a process is understood. They can be any of these things here. You've heard about examples of natural analogs, calculations, sensitivity analyses, observations from site characterizations, from experiments, and independent modeling studies done by other entities.

When we embarked on this exercise of developing the SSPA and worked on developing multiple lines of evidence, we focused on areas of uncertainty, particularly with respect to the operation of the processes over long spatial and temporal scales where we don't often have the kind of evidence from even long-term tests that we would like to have. From these discussions, we compiled a list of potential multiple lines and a small team of people working on these for each of the subsections of the report. In doing this, we found that there were benefit of using both quantitative and qualitative multiple lines. Sometimes, in the absence of quantitative data, you can still get an understanding of how a process operates.

Now, the weight of evidence from the multiple lines varied widely from one process model to another. An example that I would give here is that we had quite a few lines for the percolation flux, but less convincing in terms of the weight of evidence for lateral diversion in the PTn. Sometimes, furthermore, if a model demonstrated no effect due to a process and here an example is the lack of effect of the coupled thermal, hydrologic, and chemical effects on transport, it was difficult to find multiple lines if there was no effect.

Now, I'm going to take you through--having gone through that sort of process oriented argument, I'm going to take you through three categories of examples. First, I'm going to give you an example of where multiple lines of evidence seem to support our model, although inconclusively at this time. Then, I'm going to give you an example where really having an absence of evidence in a way provides support for process. The third example will be a potentially opposing line of evidence. The example I'm going to use for the supporting has been brought up, on occasion, today and yesterday in Bo's presentation and in others on transport. This is the effect--I'm sorry not on transport, on just the thermally coupled chemical effects on flow. As you know from what you've heard, there's been no permanent change to the flow fields and parameters during either the higher or the lower temperature operating modes. And, furthermore, with the lower temperature chemistry, it's quite similar to that of the ambient temperature.

Now, what we looked at for multiple lines of evidence in this case was both examples from natural analogs, from both at the site and elsewhere in the unsaturated zone from laboratory experiments. And, here, where we have examples of analogs that are along shallow, intrusive, and extrusive contacts in the unsaturated zone, these support the idea that there's been a localized effect of the temperature on rock-water interaction, localized alteration, and that these are very close to the contacts, not only localized, but localized close to the contacts.

The Banco Bonito and the Grants Ridge intrusion are reported in various AMRs and also in this report. I'm going to talk a little bit about the Papoose Lake Sill at Paiute Ridge. That's one that we're looking at both with field work and with modeling this year. And, here again, we seem to have a localized effect of this sill that intruded in the unsaturated zone that has been determined from nearby drillholes to have been at about the depth in the Rainier Mesa tuff as the level of the repository, roughly 200 to 300 meters deep. Now, these are all examples in the unsaturated zone. If we looked at geothermal fields also--and, of course, geothermal fields, we're talking about larger scale hydrothermal process, active processes, whereas these are what I would call fossil hydrothermal examples. And, of course, the Yellowstone geothermal field is in a saturated zone, but we selected one particular core to study that had lithology very similar to Yucca Mountain and Ash Flow tuff and where we could look at what happened between the conductive and the convective regimes. The information from this suggests that silica sealing may have formed in response to transient boiling events associated with depressurization.

I'm showing this as an example of the type of alteration that we see at Paiute Ridge. Here's a hammer for scale. These darker pink areas are the areas where alteration has occurred along veins. The whiter areas are the matrix. Although there is a little bit of alteration in the matrix, you can see a fairly sharp contact here and these anastomosing veins occur within eight feet from the intrusive contact of the sill and we don't see that kind of alteration beyond about 45 feet away from it.

I mentioned in the previous bullet regarding Yellowstone the influence of boiling. And, boiling is, of course, one way in which you can get precipitation of silica or other minerals. You can also get it through saturation increase and then evaporation. But, in these closed-system hydrothermal flow-through experiments that were done some years ago by Livermore in the Topopah Spring and Bullfrog tuffs, they showed a small reduction in permeability at these temperatures in the absence of boiling. And, of course, this was a closed-system experiment. Another experiment that is going on, but has been completed with regard to a single fracture at Berkeley, showed that tuff dissolution and precipitation in a boiling unsaturated fracture experiment indicated that there would be localized zones with elevated flux rates within the boiling front that would be most susceptible to self-sealing. And, that only small amounts of total porosity reduction are required with narrow apertures along the fracture to seal a fluid conduit. And, these statements are statements that I would say have to be tested out also with experiments on using multiple fractures and this is work that we're getting started on with a large block of tuff. It will be going on this year and next year. So, the examples from the natural analogs seem to support the idea of localized alteration along small amounts where you could have sealing of fractures, but not necessarily affecting the entire flow system.

I want to give an example now along something entirely different where we saw absence of evidence which was really, in a way, supporting evidence and this was seepage enhancement, both resulting from rock bolts and from rockfall and, essentially, we didn't see any seepage enhancement from rock bolts. There are observations of occasional dripping along rock bolts in the ESF, but studies of the chemistry of the dripping water indicates that it's probably condensation and not actual backgrounds percolation seepage. Seepage enhancement along rock bolts in saturated tunnels doesn't really apply in this case because here the tunnel is not a capillary barrier. So, we don't have any seepage enhancement from that cause. And, likewise, when looking at Rainier Mesa, Altamira which is a cave in France, Mitchell Caverns in California, and also sealed segments of the ESF in the cross-drift, we don't see seepage enhancement from rockfall.

Now, here's an example of potential opposing line of evidence and this particular example is not included in the SSPA, but it's one that we're working on now and it's been brought up several times as example of a lot of seepage. This is tunnels that have been drilled from the Santa Barbara Water District in the Santa Ynez Mountains in the 1900s and again in the 1960s that provided history of rock-water interaction. And, along these tunnels, stalactites of calcite have precipitated and they provide possibly a 100 year climate record. Here's a cross-section going from north on the right to south along the Santa Ynez Mountains and the length of this tunnel is approximately 6 kilometers long. The distance of flow paths through the Santa Ynez Mountains, it varies anywhere from 200 to about 600 meters. But, if you notice something right away, you'll see that we have here almost vertically dipping beds which is very different from at Yucca Mountain. These are sandstones and interbedded siltstone and shale and so forth. The flow pathways occur along these bedding planes and occur along a couple of faults that are found along here. Now, although the water table isn't shown on this cross-section, it's felt that the seeps are from groundwater and that wells at various locations in the mountains indicate, at least, perched water horizons that are providing the water for the seeps.

So, those are my three different examples of categories of types of multiple lines that we used. This list is just a quick list of the different kinds of analogs and calculations and so forth that you will find throughout the SSPA varying from chapter to chapter, of course. For example, I used literature studies more heavily and we talked here about corrosion as being an example of that. Microbial uptake of colloids, a lot of that came from the literature. Observations from lab and field experiments were mentioned to you today. All of these are examples that you can find in the report.

So, what we learned from this exercise was a useful way of capturing previously reported work that we hadn't really identified, per se, as multiple lines, but that to a large degree had been part of the site characterization effort. It did stretch our imagination as far as what kind of analogs we could use and the type of, let's say, confidence or certainty in the mechanisms to explain those analogs that we could use to provide our multiple lines of evidence. And then, it also gave us some ideas of where we need to strengthen our arguments in multiple lines and we're working on those for the future. So, the SSPA was a work-in-progress and we're going to continue working on the ones that we didn't get to. These will be included largely in the natural analog report if they happen to be analogs, even self-analogs for Yucca Mountain. We're including natural analog work in all of the process in all of the process models for this next year. We touched a little bit in this meeting on analogs for the drift shadow zone and we have several candidate tunnels that we're looking at for that possibility and also analogs to engineered barrier system processes. So, we'll continue to evaluate both supporting and opposing lines.

I think that's the last one.

CHRISTENSEN: Thank you. Members of the Board?

REITER: Ardyth, this is Leon Reiter. We visited Pena Blanca a short time ago. Where do you see Pena Blanca playing a role now and in the future about the lines of evidence?

SIMMONS: Well, our ongoing work at Pena Blanca is directed towards providing greater confidence in the unsaturated zone transport model of radionuclides. It will also help us to gain some understanding of transport in the saturated zone, as well. But, in addition to that, we believe that there's utility in looking further at the secondary mineralization of the uranium minerals and their potential for sequestering daughter products and that could provide some multiple lines of evidence or analogs for waste form processes, retardation with the waste form. You heard today, for example, about the reduction of neptunium solubility in the waste form and certainly getting an understanding of the way that the uranium minerals in this narrowly confined ore body have been able to remain in place for millions of years will provide some confidence in that.

CHRISTENSEN: Other questions?

(No audible response.)

CHRISTENSEN: Ardyth, thank you very much.

Now, I'd like to invite Bill Boyle to bring all of this to a fine point with his conclusions.

BOYLE: Thanks for this opportunity to wrap everything up and I'd also like to thank Professor Bullen and Professor Craig for their comments earlier today about the impressive amount of work done in the short time on the SSPA. Some of the people responsible are here today, but I'll try and make sure that the others, the many people that did it are aware of your comments.

I am going to try and be brief and leave time for discussions. I'll try and summarize what it is we had hoped to communicate. Now, if Volume I is 1300 pages and Volume II is another few hundred pages, we've had the task of communicating four or five pages per minute which is a challenging task. There's a lot of material there. So, in the next 15 minutes, I'll try and summarize it.

You heard from Steve Brocoum yesterday that put the SSPA in perspective with other documents and there was the comment by Brett Leslie of the NRC yesterday that I'd like to briefly touch upon. You know, Steve and I both yesterday gave an indeterminate time frame for when these volumes would be done and that's simply because they're in review and we want to make sure that they're done well and we don't want to set an arbitrary date and just get a product that fits the date. But, as soon as they're done, we will make them available. Volume I will be finished first, and then a short while later, a week, two weeks, who knows, a few weeks, Volume II will be available. Then, although the documents weren't laid out around the four priority areas, they certainly had a lot of material related to them, and in the four talks we just heard, you heard how the SSPA addressed them and I'll also briefly touch upon the continuing work.

Next slide. So, this is what I showed yesterday. These next five slides are the exact same ones I showed yesterday morning. This is what I said I hoped we'd get across and I hope we did get across with respect to the uncertainty and conservatism. We've broadened our uncertainties post-10,000 years. We said we were conservative and we were, but we've also changed our model, and with respect to the TSPA/SR, before 10,000 years, we were non-conservative with respect to the prior model, but we're certainly conservative with respect to the regulation.

Next slide. You've certainly heard from Bo and Bob MacKinnon, most recently Jim Blink, that there are differences, observable differences, at the subsystem level, but at system level, the high temperature and low temperature seem about the same.

Next slide. You heard from Jerry Gordon and also earlier today from Greg Gdowski and Joon Lee. The SSPA documents this framework for the passive film stability. It documents new information with respect to stress corrosion cracking and aging and phase stability. You also heard how we included a model of temperature dependence indicating progress on the fundamental understanding of corrosion and we're still doing work, as Jerry Gordon mentioned.

Next slide. And, you just heard from Ardyth on the use of multiple lines of evidence which I had indicated yesterday I always think our scientists and engineers did it and maybe didn't document it well, but the SSPA certainly is a first attempt at being very explicit about the documentation of multiple lines of evidence and you just heard from Ardyth that we'll do it in future documents.

Next slide. And, this is the work that will followon. I mentioned that this work would be done during the summer in the autumn. This work is ongoing and we should have some input by the end of the year. This will be ongoing for as long as we're working here.

Now, given all those hundreds of pages of the SSPA, nobody wants to carry it around like Rob Howard does, I can tell you that. What should you carry around and can I have Kevin's Slide 9? You've seen this plot. Jim showed it and Mike Wilson showed it, Peter Swift showed it, Kevin showed it. So, you might guess go to Chapter 4 of Volume II when it comes out because it summarizes the system level. Everything that we put in is captured here or here. These figures right here actually shed light on all four of the priority areas with the possible exception of the multiple lines of evidence, but I'll deal with that. With respect to the conservatism, you can see the difference in curves, whether you go this way or that way. Also, you know, the non-conservatism, if you will, here. With respect to the temperature mode, even though we saw differences at the subsystem level, at the system level, if the means are largely the same, you know--Mike said this afternoon, don't read too much importance into some of the differences. I think Peter said it yesterday, as well. You also see in terms of the horsetail plots, they look a lot the same. Jim even joked that he might have switched the titles, they look so similar. With respect to the corrosion priority area, these differences out here, people believe are caused by differences in corrosion back here during the thermal period. So, these charts provide insights in all three of those areas. They relate to multiple lines of evidence in that we're using the multiple lines of evidence to gain confidence in the parts that are assembled and then run to produce these diagrams.

So, now, the Board has suggested in correspondence that a reason for going cold, a possible reason, is that the uncertainties might be less with a colder repository than a hotter one. And, if you look here, as of this moment, our model really doesn't show a difference in uncertainties. We did see differences at subsystem, but not at the system level.

Can you put up Kevin's Slide 10? And, here's another way of looking at that difference in uncertainty, these relative frequency plots for the hot and the cold. The blue and the red are the same and also the CDFs are the same. You heard from Rob yesterday. It was Page 13 of his presentation. That at the subsystem level, sometimes hotter is more certain or less uncertain and sometimes cold is more certain or less uncertain. Bo gave examples gave examples with respect to thermal seepage, although we didn't implement it in this model. The thermal seepage is a more certain case, hotter. It's that it's so hot that we just don't get any. Cooler, it's more uncertain. Whereas you heard from Jim, the crevice corrosion is reversed. To me, it's more certain cold because all you need to do is make reference to the temperatures and you don't have to invoke the chemistry to show that it doesn't exist.

So, what should we do here? You know, the Board has suggested perhaps there should be a decrease in uncertainty, cold relative to hot, and we see it in some of the subsystem elements, but we lose it at the system level. Now, there's any number of reasons why that might be. One reason might be that we're not letting the model show it, but I think there are large differences between the base-case and the SSPA, whether we show the results as the means or the results as a CDF for relative frequency. The black is certainly different from the blue or the red which demonstrates that we have--it's not the model. We can put things in the model and get the results to change. The blue and the red are much different from the black. So, it's not an inability to produce different results. It's not as if we have a flawed tool that cannot show differences. These results are much different. But, it could be that our model as of this moment, we haven't put the right temperature dependency in. Like Professor Craig brought up, perhaps if had put Bill Glassley's model for thermally coupled process, perhaps we would see a difference.

Another possible reason why we don't see a difference between the higher mode and the lower mode is that although we can see differences at the subsystem level because it is a such a complex system and the system results are measured 20km away, is that it just isn't temperature dependent. The uncertainties are so large--which are shown here or you can go back to Slide 9 and the horsetails show it--that the uncertainties are so large that whatever temperature dependencies there are at the system level, they're just averaged out, smoothed out, canceled out, however you want to see it. Now, as time goes by in the course of years and we continue to add improvements to the models in terms of new models or new data into the models, if this sort of result keeps coming up, then I think in the end I would hope that people might come to the conclusion, well, it's not that they're not putting things in the model, it's just that perhaps the model isn't temperature dependent.

Now, when Kevin brought up that I had answered a question put to me Chairman Cohon last time, the chairman actually hadn't put the question to me. He put it to Steve, but I answered it anyway. I'm going to continue in that vein today to answer a question that was put to somebody else. And, it was the question Dan Bullen put to Bob Andrews. The question was is the SSPA better? I would just--I don't think better is necessarily the right word. I think it's a good supplement. It provides more information to us. It helps us put our results, our other work in context. For example, before we did the SSPA, we had the base-case and we had the claim that it was conservative. Well, now, since we've done the SSPA, we get some indication of, well, how conservative was it? Also, with respect to the uncertainty, we use bounds. We were throwing away some of the uncertainty, and by doing the SSPA, we now get a better indication of, you know, just how much uncertainty have we thrown away by going with the bound. I don't know that better is the right word, but it's certainly a good supplement.

And, one last point and it has to do with philosophy. I think you mentioned that this might be something worth having, that small difference out there. And, perhaps, it is. If the only price to be paid to get that added benefit of the blue over the red is to have Dave Sevougian do the calculation, as valuable as his time is, perhaps we're all willing to pay that price.

SEVOUGIAN: Give me a raise.

BOYLE: He said give him a raise. If I could do so, Dave, I would. However, if, you know, this difference in performance actually comes at a real cost--like if this were a discussion of real repository alternatives--and it doesn't have to be hot versus cold, just call them blue versus red. You have to ask yourself what price do I have to pay to get that ephemeral benefit because it seems to go away. Admittedly, it's long, long, and perhaps the differences are still there out at the large times, but you'd have to ask yourself is it worthwhile to spend money today, if you will, to have that benefit and look at the benefit, particularly given the uncertainty, the widespread results. You'd have to ask yourself. I know we have members of the press here, but I don't think we have the Wall Street Journal to the best of my knowledge. But, I'm pretty sure I'd know how their editorial board would answer that question about spending society's money today for that possible benefit out there, given the wide uncertainty and the results.

So, that's the end of my philosophic comment and now I'm willing to take questions.

CHRISTENSEN: Thank you, Bill. Board members? Sagüés and then Bullen and then Craig.

SAGÜÉS: Thank you. Since you have that graph over there, I want to address specifically the issue that you're mentioning in here. I don't think that we are talking about the blue versus red at 200,000 years. I'm a lot more concerned about the black versus blue and red because that tremendous shift over there or at least a good chunk of that shift is the result of a batch of 12 experiments that were performed at the University of Virginia plus another, I believe, six or seven experiments that were performed over there and there is a set of conditions which represents a highly simplified, high idealized type of test. If those are the tests that resulted in the estimation of an activation energy for the temperature dependence of the uniform corrosion rate, if those tests would have come up just a little bit different, there's a good chance that we'll be back to pretty close to the black curve. In that case, we'll be talking about dramatic change of the repository conditions. Now, here, you talk about philosophical issues, we have something which is a little more practical. One of the old tricks that you do when you're doing research, as yo know every well, is take a certain dataset, draw the conclusion, and then take away one or two of the data points and see if you still end up with the same conclusion. If you do that with a couple of these datasets, the conclusions may change dramatically. Or if you change maybe from a glass test cell to a teflon test cell, the results may come quite different. I am concerned about the tremendous effect that just those two series of experiments have had in the overall projection.

BOYLE: Sure.

SAGÜÉS: And, I would like to see your comments on that particular sensitivity of the findings.

BOYLE: And, I'm not an expert in metallurgy or corrosion, but I'll comment on this effect in the following way. Without even doing any tests--could have used Dave Sevougian's valuable time to turn the knobs on TSPA and produce this result anyway, you know, just as an insight-producing calculation. But, that isn't what was done. You know, although it's a limited dataset, they actually used data and experts, as far as I know, interpreted those data and put it into the model and the result came out like this. Now, we're not done. Jerry Gordon, every time he speaks, you know, it's this test and that test and I'm no judge of whether they're the right tests or not, but it sure seems like a lot of tests and they are the experts. So, I'm assuming as time goes by we're going to find out is that the right representation or is it even better or is it back to the black or worse? But, this result was driven by data. It's wasn't, you know, maybe there's--people look at the same dataset, interpret it differently, as you bring out. Perhaps, those data don't really represent what people think they represent. Maybe, they were driven by some artifact of the test. I count upon the experts to figure that out and we have the expert peer review of the corrosion, ongoing tests. So, I think with time we should gain insight into where the curve is going to end up.

SAGÜÉS: Okay. Thank you.

CHRISTENSEN: Dr. Bullen?

BULLEN: Bullen, Board. Well, Bill, since you are answering questions for other people, I'll pose the same question I posed to Steve Brocoum yesterday. Based on the results of this curve right here where we're looking at these data, if you go to your Figure 3, you comment that during the 10,000 year period, the supplemental model shows that there's less--let's see, the results are essentially less conservative, I guess, if you want to put it that way, or non-conservative because you actually have dose.

BOYLE: Right, exactly.

BULLEN: But, in the supplements of the EIS, the draft EIS, you still had a zero dose at 10,000 years. Now, you've got even a very small dose. The question I asked Steve was about bounding and I think he interpreted it with respect to bounding models for the hot versus cold design and the like. But, in bounding results, does this mean that the supplement to the draft EIS is not bounding any more and will the final EIS include SSPA results as the bounding calculation?

BOYLE: I don't think Joe Ziglar is--there he is. Good. I'm not touching any EIS questions.

ZIGLAR: Joe Ziglar, DOE. Yeah, the plan right now is to use the SSPA results in the final EIS. And, while from an absolute (inaudible) it may not be bounded, it does this very, very close to zero. So, from a nominal impact standpoint, I really don't see much difference there.

BULLEN: Bullen, Board. From an environmental impact statement, you're exactly right; there's no impact. But, from a psychological perspective of zero versus non-zero, it's a different number in the eyes of the public. That's the issue.

ZIGLAR: Right. And, of course, we'll use the latest available information for the final EIS, as we have in each version, so far.

BULLEN: Okay. Thank you. One last question, Bill on Figure 7. You notice the followon work and it's always great that we ask these sort of questions, but then we always get to the point when do you quit putting information in prior to the site recommendation decision? And, I know we've got up until later this year until Is have to be dotted and Ts have to be crossed and some document has to be handed to a higher-up in the political spectrum than our pay grades, but what do you think you're going to know based on this information and how will we know it as a Board? When will we see supplements, upgrades, including the public having access to the same kinds of documents?

BOYLE: I believe you heard Kevin say that there--and I think Peter Swift said it yesterday afternoon--that with respect to putting more things into the TSPA model, we may not. Because if we do, whether it's just even one thing, we've got to go back and get Dave Sevougian and get him to run everything all over again. So, with respect to the large model, we may not add anything else and this exercise, the supplemental model, will be along the lines that Kevin said, to try and find out like, for example, the uncertainty and even the mean results and the high temperature and low temperature at the gross level are the same, but actually are there any differences between them? You know, have we sorted through like are there uncertain--the different uncertainties driven by different things. I don't think so, but we haven't done the work to find out. So, that would be that sort of work and data collection, of course, whether it's related to this or not, we always do. But, all three of these, we hope to have documented autumn time frame and referenced in Progress Report 25 which would come out by the end of the year. So, it would be publicly available.

BULLEN: Bullen, Board. Maybe this is a followon question to Kevin because I think the question I was trying to ask when I talked about the difference between the hot versus a cold design or HTOM versus LTOM was if you looked at the horsetail diagram and you looked at the actual sampling for each of them, was there a certain range of parameters that resulted in wider uncertainty in the hot design versus a wider uncertainty in the cold design because of the--I mean, you only had certain cases where there actually was part of the horsetail showing up and was that different sampling regimes that would give you an indication that there actually may be differences in the subsystem models? Will you be able to see that?

COPPERSMITH: This is Coppersmith. Yeah, I think you answered it at the end. In fact, you'll see those at the subsystem level. There will be differences that have to do with seepage flow rate or some other subsystem level and, in fact, those are called out. They're called out in Volume I and they're called out particular in Chapter 3 to Volume II. And then, what happens many times--and there's counteracting effects such that it doesn't make it into the total system. So, I think part of this effort, that first bullet, it's not really adding anything new. It's just dissecting what we have. And, even the discussion here of the dominance of the temperature dependence of the general corrosion rate, we haven't seen what do we mean by dominance? How much of the uncertainty is contributed by that particular input? It would be nice to have a better feel for 25 percent, 80 percent. It would be nice to have that. These are existing inputs. That's just another way of looking at it.

CHRISTENSEN: Dr. Craig?

CRAIG: Paul Craig. Yeah, we're at the time when we get to reflect at the end of the meeting and I've got a couple of comments, one that relates to you and the other relates to

Kevin, but they're really closely connected.

The presentations that we've heard, it seems to me, massively confirmed the centrality of C-22 and titanium behavior. Without those, you're really in deep trouble. The extrapolation of a very limited number of laboratory experiments remains key. Now that we're into the temperature dependency, we see that Arrhenius coefficients really matter and they're extraordinarily cruelly determined for reasons that were explained fairly clearly in a presentation. All of this seems to be driving this kind of massive change that we're seeing over here. In some sense, it seems strikingly late in the game to have such enormous model instability unless, of course, the modeling instability doesn't really matter because the repository performs fantastically well regardless of what model you take. And, if the instability is out in a region where it doesn't matter, well, then, okay, that's fine, all is well and good. I simply find that to be a question which I'm struggling with and don't have an answer to. But, I'm talking about communication here so my focus is really on communication in this situation where the data, particularly in the higher temperature end, seems strikingly ambiguous and that seems to me to be the most compelling argument for going with a cooler repository. It seems to be much, much more defensible in the near-term in the region where things really matter which is the corrosion of the C-

22. We haven't talked about the failure of the titanium, but presumably, there's similar discussion to be had there. The first point.

The second point connects with Kevin's presentation. Kevin has absolutely amazing ability to take complicated stuff and find alternative and new cuts which give us new insights. As Dan pointed out in the supplemental draft environmental impact statement, you had identically zero dose for 10,000 year which struck me as, really, a touch of arrogance to it. I am personally inclined to say that going from absolutely zero to 0.00006mr/yr represents a change which is important and positive. That's really a good change. What the number actually is in this particular instance is far less important than the fact that the number is not exactly zero. Now, if I look at Kevin's #10 which was up there just a moment ago, I discover that in contrast to all those runs that showed zero back in the draft environmental impact statement, we now have either 11 or 12 percent of the runs that show exactly zero dose going out for 1,000,000 years. 10 or 11 percent, that's really an interesting thing. I'm glad you told us that. I'm trying to figure out what does this mean? And, it means, maybe, that the--that's a big fraction when we think of 95 percent confidence limits. Is this an indicator that the repository is really incredibly robust or is it an indicator that something is drastically wrong? I don't know the answer to that, but as I said, what I'm trying to do is to focus on the communication side of the presentation here and raise issues which seem to me we do have some technical implications in terms of toughening up the understanding of the corrosion of the C-22 and the titanium.

BOYLE: Well, if Kevin doesn't mind, I think we'll go one-two. I don't know that I would characterize this large change as instability in the model. I don't think there's any secret within the project that I have been a proponent of adding the uncertainty. Not that I object to the bounding calculations, but in order to put it in perspective at the uncertainty. The people that put the bounds in, some of them did so, in part, just to avoid the sorts of questions that Professor Sagüés is bringing up. They felt that they didn't have a large database to justify going with a, you know--we work better with a more uncertain model. So, they bounded it just to avoid these sorts of comments. So, I wouldn't characterize it as instability. It's just the new model was done to gain insights into performance.

Now, with respect to the second point, before I turn it over to Kevin, you're saying that you actually see value--you know, just in terms of believability and Professor Parizek is nodding his head, as well, because I think he said the same thing. With respect to Dan's question on is the

SSPA better which I said, well, it's a good supplement, just from a communications point, I agree with you. That I think having these early results are better in the sense that they're more believable to you and to you and it's--people find it hard to believe that we were conservative and yet we had no releases, at all. And so, from that point of view, I view this large spread in the results or the low slope of the CDF here as a good thing. But, having said that, I'm not against in a licensing forum doing with bounds if we have this sort of information that allows us to put the bound in perspective. I mean, last week, the Nuclear Regulatory Commission based on what I read in the newspaper mentioned to Chairman Cohon that the NRC has no problems with bounds. It's an accepted approach with them.

COPPERSMITH: This is Coopersmith. Remember that very little has changed from the time that the TSPA/SR Rev 0 was done and from the time that we developed SSPA. I mean, there's been some new data and so on. But, I think what's changed most, again, uncertainty is in our minds and so it's our representation of that. We were talking today at lunch and maybe we should have called this unmodeled uncertainty. People have trouble with unquantified uncertainty. There were some things, some uncertainties and some porosises, that simply weren't modeled or weren't modeled realistically the way we expect them to happen. They're modeled in a bounding sort of way to be conservative. So, what's changed in terms of the movement here is more than a total change in our perception or new data that lead to instability if we have a new piece of information. It's actually a change in the rules. The rules were to be conservative in bounding or the rules were to be representative with uncertainty. And, they actually do coexist well because if you can use one--if an argument is going to be made for the black line being conservative, you know, have a basis for making that argument. So, it really has to do with our representation of uncertainty.

CRAIG: Yeah, that's an entirely legitimate way to make use of this. Say, yeah, the uncertainty is if we go out to 100,000 years, the uncertainty extends over six or eight orders of magnitude or whatever it is. But, even taking all that into account, you're still doing well. So, again, it's the point that if the uncertainty is in a range that doesn't matter, so okay, it doesn't matter, good. But, this is very interesting. Very interesting. Thank you.

CHRISTENSEN: We have two staff questions. Okay, one staff question. Dan Metlay?

METLAY: You almost had none, but since you brought up philosophy.

BOYLE: Dan did.

METLAY: Or since you responded in a philosophical vein, let me see if I can present you with a hypothetical. The performance on that mean annual dose curve to the first 10,000 years is now not zero. It's a very small number. Is it conceivable that with this further work that you're undertaking and the re-analyses and perhaps an extension of Professor Scully's experiments and replication, etcetera, etcetera, everything that the scientific process does, is it imaginable that that 10^{-5} result might be 10^{-3} or 10^{-2}? And, if it is imaginable and here's where the hypothetical comes in, how does your thinking about which model is appropriate, how is that thinking affected?

BOYLE: Okay. And, this sort of came up with Mike Wilson and, I think, Professor Parizek, when he was mentioning that, you know, he saw a value in reporting this. What if it were higher? And, there is that possibility. I would like to judge the models on their merits, you know, when we go to use them. If I had a feeling that this was the better one, even if it was 10^{-2}, well, that's what I would tend to believe. It's still below the dose, you know, the limit which is somewhere up here, but I can tell you one thing that might happen. Even now with it down here at a close to zero, but non-zero, number, I think there are people in the project that want to find ways to bring it right back to here. Like we had a question from the audience already. I forget--somebody, one of you, maybe you read it, Professor

Bullen, it's some people believe that even these results are too--they're unbelievable. You know, what happened to the second lid and that sort of thing. And so, it's thinking along those lines that if it were to get up--it doesn't even have to get up here. There are going to be people that want to bring it right back down to zero over here and at the same time, perhaps not realize that they're going to have to pay a price with Professor Craig and Professor Parizek in terms of then the results aren't necessarily believable anymore. You just look at them and you go, zero in 10,000 years. So, myself, as long as we stay a good ways away from the limit, I have no philosophical problems being non-zero. In particular what we have and--don't forget that the disruptive releases are up there anyway.

CHRISTENSEN: Bill, thank you and my thanks to all of the speakers for a really interesting afternoon. And, I now turn this back over to Chairman Bullen.

BULLEN: Thank you, Dr. Christensen. Throughout the course of the meeting, we've had six or seven questions that have been submitted to the Panel, but not asked. And, in light of conserving time for public comment, I'm just going to actually give them to our transcriptionist and have them appended and added to the record as if they were written comments. Unless, of course, the people who wanted those questions asked would like to use the public comment time to do so. We have two members who have signed up for public comment. I'd like to remind you that we had public comment just before lunch. So, we really are about on schedule.

Kalynda Tilges? Kalynda signed up and said that she had some written comments forthcoming, but would she also like to make public comment?

TILGES: Yes.

BULLEN: Okay. Kalynda? How about eight minutes apiece? We have two people, eight minutes to--

TILGES: I'll do my best.

BULLEN: Okay, thank you.

TILGES: Hi. My name is Kalynda Tilges, Nuclear issues coordinator for Citizen Alert. I actually missed the first day's meeting. So, maybe, I can keep it to eight minutes.

I'm not only the nuclear issues coordinator for Citizen Alert here in Las Vegas; I'm also a long time Nevadan, a mother, and a grandmother. And, according to everything I hear, I'm also a dose receptor, but I guess one of the reasons that I'm up here being a dose receptor, I'll quote from our friend, Judy Triechel, is that in our state a dose receptor is a Nevadan and a health effect is a dead Nevadan. And, I don't plan on myself or my children or my grandchildren having a health effect. So, with that mind, I could have nitpicked this whole thing apart, but that would take me way too long. So, just some comments that I wrote down.

There seems to be a lot of scrambling around to deal with the effects of water in the repository and it seems to me that Yucca Mountain was singled out. One of the main reasons Yucca Mountain was singled out was that it was such a dry area. So, this is something that is confusing to me. There's a lot of claims to know a lot about the groundwater flow in this area. However, I want to know how much you really know about the ground flow under Yucca Mountain being connected to the Frenchman Flat area of the Nevada Test Site or the Paiute Mesa because your counterpart in the Department of Energy on the Nevada Test Site doesn't seem to have very much of a clue about this. All they know is it's contaminated and, well, we're not really sure where the plume is. Maybe it's coming toward Yucca Mountain and has the Department of Energy and the Yucca Mountain Project taken into consideration the cumulative effects of whatever groundwater contamination is at the Nevada Test Site flowing into Yucca Mountain and do you even care?

The predictions and evaluation of the drip shield, you're talking about possibly leaving the repository open for about 300 years. My understanding from the SDIS is that the drip shields wouldn't even be emplaced until just before the repository was going to be closed. So, we're looking at possibly 300 years in the future, but I didn't really see any reference to cask performance before the drip shields were emplaced. Possibly, I missed something on the first day.

I'm very concerned about the TSPA. You're dealing with computer modeling. And, maybe, there's a lot--I'm sure there's a lot I don't understand. But it sounds to me the more I listen, I really try and understand, but I get more and more confused at each of these meetings and it seems to me as that you're using computer models to validate your computer models. And, as I brought up before, you're doing your work on regulations that haven't even been finalized or adopted yet and not even following the physical qualifying and disqualifying characteristics of the mountain that are in place in the regulations and laws that are on the books now. And, to the Technical Review Board and Danny Bullen, I really appreciate you guys being here and trying to hold the bar up for the DOE and being an advocate for the public, but it just--I don't understand how the Department of Energy can be allowed to continue in what seems to me is an entirely illegal vein. It seems to me that they should be working with the laws and the regulations that are on the books now. When the proposed changes are made, then you can go ahead and start working toward that.

Kind of on that subject, there seems to be so much uncertainties. I asked this question at the Technical Review Board meeting back in Amargosa and I know it sounds very facetious and cynical and I really didn't mean it to be that way. I have to ask it again and again I don't mean to be cynical. But, as I listen to you, the only way I feel I can word this question is with so much uncertainty, how is it that the Department of Energy and the Yucca Mountain Project can be certain that you're going to be certain about this project by the time site recommendation comes up? Which brings up a whole other Oprah in the fact that you're talking about if, when, maybe, we hope, possibly, five to 10 years more studies. It seems to me you're just barely scratching the surface of bare minimum of perception of what's really going on out there and yet you're planning on site recommendation this year. This has putting the radioactive cart before the horse. It's entirely illogical. It's completely offensive to the people not only of Nevada, but this country, I think. I think you do us an injustice. And, I think there are quite a few people in this room who are familiar with the Darwin Awards and I feel that there's going to have to be a whole new award category for the Department of Energy when they wipe out the entire human race starting with Nevada.

Asking Abe Van Luik at the STIS hearings how much this has cost, so far--I'll try and wrap it up--he said, oh, seven billion, give or take a billion. Well, you guys are playing fast and loose with our money here. So, it seems to me that money should not be the bottom line on our safety. If it takes another billion or so to make sure we're safe, to get rid of these uncertainties, spend the money. These are our lives.

This is a project by the Department of Energy's own admission is going to fail and yet you move on. You say you don't want to set an arbitrary date, but you still have years and years of studying. This is your quote; you don't want to set a arbitrary date. Maybe I've paraphrased a little, but you've basically done that by setting site recommendation for this year. And, I say you in the grander scheme of things.

As far as we're concerned--when I say we, the people of Nevada, the farming community out in Amargosa, 12 miles away from your project that's guaranteed to leak and fail around which you won't be anywhere near, neither will your children, we talk about the only 100 percent success rate that the Department of Energy has and that's of contamination of very site it's ever had in its control. And, yet, you stand here and tell us we'll do it right this time. I try hard not to be a skeptic, though I'm sure my husband would be more proud of me if I was, but even I don't believe you.

I consider risk assessment a voodoo science and it's basically just a PR approach to making the public feel better about being murdered. And, I guess there's really no wrap-up to this and maybe I sound a little emotional. I really try hard to keep that out of my testimony. I really try and be clear and unbiased. Not having had much sleep running around at different meetings trying to chase you all around as a single organizer in the City of Las Vegas is very difficult. But, it's a responsibility when I look at my children that I have to do. And, maybe I'm a little grumpy, but after listening to you all, I realize I have a right to be and I wish there were more people from Nevada here to tell you how they feel because I think they'd be a lot grumpier than I am.

Thank you.

BULLEN: Ms. Tilges, thank you very much for your always through-provoking comments. I think maybe after two days of hard meetings, we're all a little grumpy. But, again, thank you very much.

Our last commenter that's signed up is Mr. Tom McGowan. Mr. McGowan, it's all yours?

MCGOWAN: I would like to begin in the highest tradition of Las Vegas entertainment by recognizing a dignitary, a visiting guest artist in our midst this evening. You may not be aware of this, but he is a superb exponent of the limbo and I won't reveal what he had on his head other than a lampshade on a particular event. But, in any case, he's a fine gentleman and I consider him and I hope he considers me his friend. His name is Dr. Steven Hanauer from headquarters. Hello, Steve, how are you?

HANAUER: Hello, Tom.

MCGOWAN: He made the astute observation that when I left the Alpine Village Inn, they tore it down. Actually, they were tearing it down at the time which is why I left.

And, Dr. Craig, how are you? Another visiting dignitary. Where were you yesterday? Don't answer that. Had long lunch breaks.

Seeing I have this extensive amount of time, I'll try to throw something together here. I was late today, but I brought a note. Here it is. This one and a half day meeting of the Joint Panels of TRB sequentially chaired by Dr. Bullen, Sagüés, and Christensen easily ranks as among the best conducted and perhaps most probing and loose data meetings since the inception of the TRB. That's meant very sincerely. Not to be denied, the eminent roster of DOE, OCRWN, YMPO representatives, their M&O contractors, and the expert (inaudible) from national and scientific laboratories exhibited an unprecedented degree of cogent articulation and timely responsiveness to a session inquiry under stressful conditions and with relative ease except for one anomalous departure from the norm of self-confident excellence who will remain unidentified. Well, I will leave that for you to cope with unraveling that mystery over one or more sleepless nights if you choose. Quantified by the realization that human means imperfect. Hence, nobody is perfect except me, of course, which proves my point, I think. Are we together, so far? Good, we have 11 minutes left or one? It keeps going up, doesn't it? There's no gambling in the State of Nevada because you didn't understand that. I hope before you go out there and go ape, make sure you lock it away somewhere.

That gives rise to the correlated fact that there's no such thing as almost pregnant, almost perfect, almost guaranteed, safe, secure, and human intrusion impervious underground repository for the permanent storage of high-level nuclear waste. Which latter event is the more disconcerting and humanly repugnant in view of the inevitable consequences? Since permanent means immobilized in place essentially forever which is not, cannot, and will never be the case. I didn't hear anybody ask that particular question, but there's the answer in case you think of the question one of these days.

A 1940's classic motion picture tells of the Oxbow Incident starring Henry Fonda. It was based on a historic event which took place right here in the (inaudible) pioneer state of Nevada and told of an itinerant ranch hand who arrived in a small town and happened upon a lynch mob about to execute three men who were falsely accused of theft, rape, and murder. When Fonda, who played the stranger in town, voices objection to the injustice taking place, the leader of the lynch mob gruffly told him to mind his own business whereupon Fonda replied, "Hanging is the business of any man who is around." And as that sinks in, as it occurs, so is this. Whether you're willing to accept the fact or not, that's what this is, which explains why I'm here in the adversarial role.

I love you dearly, but I may not necessarily be with you for the rest of time. But, the most important question is why are you here? What do you do here exactly? By what testifiable reasoning basis? Notwithstanding to deserve the claim expressed at the beginning of my public comment which I deleted for the sake of time and sensibilities, I think, did I say something nice about DOE inadvertently or on purpose? I'll say it on purpose. Okay. (Inaudible) this Board. You're all swell people, but that's not what this is about. It's about life or death, isn't it, swell people or not.

And, I was thinking (inaudible) inadvertently, you've missed the point completely both on scientific and technological grounds and the chairman is responsible for compliance with the higher (inaudible). For example, you exquisitely detail and address the underground geophysical properties, dominance, sub-molecular minutia, burden it to the SSPA in support of a defensive depth of that which is apparently indefensible, otherwise. Fails to disclose the (inaudible) facts that (a) it's impossible to assess a three-dimensional iteration of a four-dimensional universal space/time continuum, or any sub-locally site-specific and limited by that incremental segment of it; (b) nonradioactive mummies, archeological artifacts, natural (inaudible), and isolated tunnels are irrelevant to the subject (inaudible) study and (inaudible) and inapplicable as independent lines of evidence since they do not contain a radioactive source for the propagation of thermal energy impacted upon the surrounding host rock over any substantive term.

Maybe I'm mistaken. Is there a radioactive mummy or not? Pardon me, I couldn't hear it for the public record.

BULLEN: Not like this.

MCGOWAN: Was that a uh-huh or what?

BULLEN: Not like this.

MCGOWAN: No, thank you very much. Then, why was it brought up? Don't answer that. That would be perplexing; compounding, I should say.

It's elementary, this (inaudible) formally impacted nonhomogeneous underground host rock matrix securely combined with a cooler, hence rigid, intermediate and far-field rock matrix (inaudible) regional area mass and tensile integrity can only expand within and upon itself. And, any fractures (inaudible) and other discontinuities or voids within it including a contiguous (inaudible) repository drift (inaudible) to a metastatic state wherein the slightest imbalance impetus can and will trigger the underground, over-catalytic, spontaneous, explosive, shattering, and disintegration of itself and everything in it. I didn't hear anybody mention that. But, why bother?

Consequently, an underground repository of a high-level nuclear--I've got about five minutes left. Is that okay? I'll shorten it, though. Consequently, an underground repository of high-level nuclear waste is an analogous to the emplacement of a thin-shelled or unshelled soft-boiled egg within (inaudible) encompassing jaws of hydraulic press attained to a metastatic state, a prompt explosive, self-destruction, (inaudible). Which proves that the (inaudible) pathway is the one between the years (inaudible) singularity. But, why quibble when the time is of the essence, excited haste, (inaudible) leisure, and rather than remain silent and be that ignorant, provide exhaustively detailed oral presentations, charts, graphs, and gesticulations and remove all remaining doubt. When is your next meeting? I don't want to miss it.

In the immortal words of (inaudible) beyond reasonable refuted conjecture, who has met the enemy, it is us. But, it's time we understood and accept that fact in a spirit of humility. Most ironic and incongruous, that name is indicative of attainment to higher achievement, such as Bullen, Parizek, Sagüés, Wong, Christensen, Cohon, Nelson, Craig, and so many others in (inaudible) of perception, however circumstantial, but inextricably and contingent upon your fully informed advisory recommendations to the Congress and President of the United States, are now poised to either assume your rightful place among the truly great or (inaudible) time or (inaudible) be relegated to the (inaudible) scrap heap of human history as having failed utterly yourselves, each other, and all posterity.

In the latter instance, it's irrefutable that whether wittingly or unwittingly, the official generic "you", inclusively, has engaged in conspiracy to commit mass genocide on a historically unprecedented human and universal scale, inter-generationally, and in perpetuity by the underground emplacement of toxic radionuclides inevitably insure deadly impact upon human and all other species (inaudible), as well as the natural environment, rather than sustain life and there as potential as (inaudible) human itself.

To conclude here, that gets a standing ovation as a rule. I'll be about another 35 seconds, give or take a decade. Okay. In relevant side note--and this may smart just a little bit, but you're big guys and gals. A relevant side note here, Eichman never personally forced anyone into a heinous gas chamber or an oven. He simply followed orders and signed the directives for his subordinates to subsequently carry out the (inaudible) solution in (inaudible), such as Auschwitz and (inaudible). But, upon the end of World War II, the intimidation was tribunal at Nurembourg (inaudible) at the following of an immoral order and the mere fact of separation by time and distance, premise ultimate consequences, was not a competent legal defense. (Inaudible) of a mass genocide of the millions of innocent, unsuspecting and defenseless men, women, and children, and the death camps of Nazi Germany. The only difference between Eichman and the official generic "you" is that although there is activities where (inaudible) in terms of geographic area (inaudible) demographic victimization have been temporarily been laid, but inevitable. Impacts of consequences of your official acts, omissions, and advisory recommendations are historically unprecedented in human and universal scale inter-generationally and in perpetuity for the rest of human time. This wraps up some.

But, there is a window of opportunity for you to exhibit the integrity and intestinal fortitude to take at least one, however timorous and (inaudible) step down from the (inaudible) tree and together--together, in community, stride competently across the nonreturnable threshold that opens onto the brilliant horizon of challenges and opportunities through extraordinary human achievement which awaits and beckons throughout the third millennium and beyond. You may not believe this, but I'm confident you will choose the positive course toward the safe and sane future in sight of almighty God. God willing, I hope to see all of you there.

You can have the rest of the time, Dr. Bullen, for the party or whatever the hell there is you have in mind. Thank you very much. Don't get up, anybody.

BULLEN: Thank you, Mr. McGowan. You always provide us with thought-provoking comments. And, that last come, too. That's exactly right.

Before this meeting closes, actually maybe I should answer a question that was raised and I'm going to get the date wrong, but we're here the second week in September for a Full Board Meeting. Is that correct, Mr. Executive Director? Dates are on our web site. I know that. And, the other question is it's at this hotel, right? Check the website for these activities. I think it's the second week of September, Full Board Meeting, and it is here.

Dr. Parizek?

PARIZEK: 10th, 11th, 12th, 13th, 14th including travel dates.

BULLEN: It's got to be 11, 12, because we don't have meetings on Saturdays.

PARIZEK: Public would be 11:00, I see on my listing.

BULLEN: Okay. Next meeting and it's here.

Before we close, I have to express some deep appreciation from the Board. We had outstanding presentations all around, as also pointed out by Mr. McGowan. I'm not sure which one wasn't good, but we'll have to think about that and toss and turn at night.

I also want to express our appreciation for the direct addressing of the Board's issues and concerns. Leon Reiter and I had a number of conversations with Claudia and with Bob Andrews and people about what we wanted to see in this meeting in the presentations. And, we were very pleased or I am very pleased with the results that we saw.

I also want to acknowledge that it was extremely difficult for you to, one, bring this meeting together in the time frame allotted, but also for you to provide information that's still a work-in-progress. We realize that it's still a work-in-progress. Even though you've been successful in compiling over 1300 pages of information that I have yet to go through completely, we really understand that it's crunch time, you're doing the best you can, and we are really trying very hard to keep up. We want to thank you again for keeping the meeting on schedule, for providing us with the great presentations, and for giving us information in a very, very timely manner.

With that, I'll ask if there's any other comments or questions that should come from the Board or staff?

(No audible response.)

BULLEN: Seeing none, I declare that this meeting is adjourned. But, before I leave, I want to remind the Board members and staff members we have a debrief meeting starting at 5:30 on the fourth floor. Be there.

(Whereupon, the meeting was adjourned.)